The British Journal for the Philosophy of Science

Volume X May, 1959 No. 37

SPACE *

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A past chairman of this Group, Professor Woodger, has a capacity for saying profound things lightly. I should like to quote one of them. It occurs in his book *Physics, Psychology and Medicine.* He is discussing some prevalent errors concerning the nature of reality and says—

> ' Another tenet of this philosophy seems to be that everything that is is in a big box called Space, which is floating down a river called Time. Consequently if anything (except the river !) is not in space, it is just not at all.'

This is the philosophy that Professor Woodger rightly invites us to reject. He might have issued the invitation with a parade of learning and abstruse logic; but the uncompromising simplicity of the form in which Woodger presents this erroneous view of reality embodies a criticism of it by implication that is both more cogent and more precise than a learned dissertation could be.

We are all prone to adopt the big box view of reality and we have to understand why it is wrong. It contains two errors.

The first is the assumption that only those things with location have reality; that anything not in space is ' just not at all ', or, more shortly still, what is nowhere is not. ' Nowhere ' and ' non-existent ' are regarded as synonyms, and according to this school, space itself is not necessarily real but everything is real that is in space and nothing, except perhaps time, is real that is not in space.

There are, of course, those who would like to adhere to this school and yet doubt whether it is correct to identify ' nowhere ' with ' non-existent '. With the leaning towards compromise of the woolly-minded they then prefer to identify it with ' not quite '. In other

* Chairman's Address to the Philosophy of Science Group, 6th October, 1958.

words they introduce the view that there are degrees of reality in the same way as there are degrees of temperature or degrees of hardness. What is nowhere is not *quite* real, they would say. Some things are to them more real than others. According to this remarkable interpretation of the concept ‘ real ’ they say that particles are the most real of all. These they think of as consisting of a substance, which they might call particle stuff. Particles are regarded as interacting with other things that are several degrees less real, such as waves. The very real particle stuff and the not so real waves are then regarded as having their existence in time, which is hardly real at all. They are also said to have their existence in the big box space, about the reality of which they do not care to think. Mass and electrical charge, being properties of the particle stuff, are placed high on the reality scale. Extension, being a property of space is placed low.

If the course of events is ever subjected to control it is taken for granted, by adherents of this school, that the controlling influence consists of particle stuff. Having heard of servo-mechanisms and feed-back they say that the controlling influence must necessarily be a part of a closed loop and, moreover, a part consisting of material substance. They find a place for it in the big box. As nothing is believed to exist unless it has location in the big box it is regarded as unscientific, bad philosophy, or logically absurd to postulate a controlling influence without location.

Be it admitted that all things possessing *physical* reality do occur in the big box. The error is to assume, indeed to take it for granted, that physical reality is the only kind of reality. I have myself attacked this somewhat fashionable but, nevertheless, naïve view on many occasions and propose to leave it at that for the moment. I want to pass on to the second error implicit in the big box philosophy.

The second error concerns the nature of space. The view that space is a container was held almost universally until early in this century. It is true that some denied it, among them Leibniz, and some doubted it, among them perhaps Newton. But since the success of Newtonianism the doubts were never clamorous. When it was said that all physical things are *in* space I do not think that it would have occurred to many to question the appositeness of the preposition ‘ in ’.

Yet doubts ought to have arisen. If space were the container of physical reality it would be relevant to ask how full is was. How much reality must be put into a given region of space so that it may be 100 per cent full? How much would make it 50 per cent full? Do the actual contents of space fill it up to just as much as it can hold? If something more came into space than is there already would reality overflow and spill into the river Time? If the big box were filled with radiation only, how much would there be to fill it? How much particle stuff, alternatively, is needed to fill the box?

I hope such questions seem as absurd to you as they do to me. What I want to make clear is that we ought not to have had to wait for relativity theory before we abandoned that queer notion of space implicit in the big box philosophy.

Be that as it may, it was Einstein who showed more cogently than anyone had done before why it is wrong to regard space as the container of reality. (But it is only fair to add that Newton seems to have had his doubts about it, and Leibniz certainly did.) When attributing physical properties to space Einstein obliged us to replace the word ' container ' by the word ' constituent '. Space, we learnt from him, is a constituent of the material universe and not its container.

We learnt if half a century ago. But we are in danger of forgetting it. Today general relativity occupies only a very small part of the undergraduate teaching in physics and astronomy, if it is taught at all at that level. Post-graduate studies are necessarily devoted to other, usually more recent, subjects. Even special relativity does not receive the same attention from scientists today that it did in the 1920s. It is found that a physicist or engineer can do all that he needs when working with particle accelerators if he has familiarised himself with the relativity equations and the rather specialised notation that they employ. Pre-occupation with meaning, such as was common in Eddington's day, is no longer necessary. It suffices for practical purposes to know how to use the letter symbols.

The academic syllabus does not have room for everything. Very few physicists and astronomers are engaged on work that calls for any knowledge of general relativity. Those few who have made a deep study of the subject have been stimulated by intellectual curiosity rather than by practical needs. Nevertheless, I have reached the conclusion that those few may be able to contribute much useful knowledge. Let them be encouraged. Perhaps it is just because I have little specialised knowledge myself that I view the progress of science from a little distance. What I see leads me to believe that some aspects of the physical world can be more profitably explored by one who takes as his starting point the position that was occupied in the 1920s than by one who sets out from the position more usually occupied today. In Eddington's time the focus of interest was the interaction between space and ponderable matter. That subject is rarely discussed today. Now the focus of interest is more often the interaction between particle and particle. So I want this paper to serve as a reminder of the great work done by Einstein, Eddington, and others. I propose to suggest lines for future study that are stimulated when one places oneself in thought in the atmosphere of the 1920s.

Let me draw attention to another of the defects in the pre-Einstein view. This view obliged us to think of every bit of ponderable matter as having two environments. One of these was called ' space ', the other ' luminiferous ether '. The two environments were said to be quite distinct. Most of us regarded space as featureless, as such that any one part was in every respect exactly the same as every other part. The only property that could be attributed to space viewed in that way was extant, but even that property had little meaning, if any, so long as the extent was considered to be infinite.

In contrast to the featureless space we thought of the luminiferous ether as featured. Its various parts were said to be subjected to a variety of strains. These strains were considered to be electrical and magnetic. For some reason gravitational strains were not postulated. Gravitation was regarded as a phenomenon about which the less said the better.

To explain the presence of the various strains a variety of physical properties was attributed to the ether, such as elasticity, density, inertia.

One of Einstein's great contributions to science was to show that there is no need to postulate two environments. One will do. I think this was as great a contribution to philosophy as to science.

It is now recognised that a space of which no part is distinguishable in any way whatever from any other part is not observable by any physical means. Its nature is, therefore, purely conceptual. It is difficult, if not impossible, to argue that a featureless space has any physical meaning. Things would happen just as they do with or without a space of that kind. Of the two environments assumed for every body, reality could be assigned to one only.

This reasoning could have left scientists with the luminiferous ether, thought of as differing from one place to another, as being featured; and the features being considered such as to influence measuring devices, and as to be observable.

The measurable features, in the environment of ponderable matter, all have something in common which can be expressed by a single word, namely, field of force. Such fields are measured and described in terms of their intensity, which is more precisely called their potential gradient. There are at least three kinds of field, electric, magnetic, and gravitational; so the environment of a given quantity of ponderable matter may contain three different kinds of potential gradient. Each of these may vary both in magnitude and direction. At least the electric and magnetic potential gradients may also vary with time at any given point. When they do so they are called waves. The physical effect on ponderable matter of these potential gradients is always of the same kind, namely, that of accelerating the ponderable matter.

When this insight was gained a name was needed for the collection of potential gradients that surrounds any given quantity of ponderable matter. ‘ Luminiferous ether ’ might have been retained for this were it not that its verbal currency had been depreciated by sundry misapprehensions. Hypothesis had, for instance, endowed the ether with the properties I have already referred to; elasticity, density, inertia. But no such properties could be attributed to places where there was no ponderable matter. The only observed, or even inferred, physical features of the environment were various potential gradients, some static and some alternating, some bound to a particle and some moving freely with the velocity of light; and so the many properties that had been attributed to the ether proved both inappropriate and too numerous to make this term suitable. If the word ‘ space ’ suggested something too abstract, the words ‘ luminiferous ether ’ suggested something too concrete.

Perhaps the non-committal word ‘ environment ’ might have been a good choice. It could have been justified scientifically; for it is strictly accurate to say that a quantity of ponderable matter is surrounded by an *environment* that has physical properties and one has to be very cautious about saying anything more than this. But to use ‘ environment ’ in this way would have been to raise the word to the status of a technical term and to use it, moreover, in an unfamiliar context. ‘ Collection of potential gradients ’ could also have served for these are, literally, all that one needs to postulate as occurring between one bit of ponderable matter and another. Whether the container of the potential gradients be called space or luminiferous ether is immaterial. The point is that the concept of a container is unnecessary. The potential gradients are observable by their effects. But, everything would happen as it does whether these gradients have a container or not.

But neither ‘ environment ’ nor ‘ collection of potential gradients ’ would ever have succeeded in ousting the established word ‘ space ’. So I think that Einstein showed a sound intuition when he retained this word while changing its meaning from that of a featureless container to that of a synonym for featured environment. But when the word ‘ space ’ is used in relativity theory with its new and precise meaning, it does not at the same time represent the vague concept that it does in everyday speech. It has become a technical term.

Another piece of insight gained from relativity theory was that the properties previously attributed to the luminiferous ether could not be attributed to the environment of ponderable matter. It appeared at least possible that they could all be replaced by one single property described as ‘ curvature ’. It was from the identity of inert and gravitational mass that Einstein arrived at the conclusion that a field of force, at least when the force is gravitational, is a region for which the geometry of space is non-Euclidean, and he used the expression ‘ curvature ’ as a means of describing the departure from Euclidean geometry for the space in which there is a potential gradient. This, too, became a technical term with a unique and precise meaning in relativity theory.

The conclusion startled the scientific world. Until then we had regarded curvature as a purely geometrical property. That it could be regarded as a physical one caused scientists to revise their notions about the nature of matter. How, it came to be asked, could something as apparently abstract as a curvature of space interact with something apparently as concrete as a material particle? The question is as puzzling today as ever it was.

The best answer will eventually come, I am inclined to think, from a revision of our notions about what is abstract and what is concrete. In making a distinction between these we tend to be enslaved by the organs of sense perception with which we, as human beings, happen to have been endowed. We call those things concrete that we can perceive with the help of these organs and those things abstract that cannot be so perceived. But this is a surmise and I do not propose to pursue this line of thought any further here.

Relativists have succeeded in showing good reason why gravitational potential gradients should be identified with a condition of space appropriately called its curvature. But they have not yet succeeded in showing the same for electrostatic and magnetic potential gradients, though some of them still hope to do so. The effort to do this is called the search for a unified field theory. Anything that I, or anyone else, can say today about the relation between space and matter may have to be modified if and when the search has succeeded. For this reason, if for no other, whatever can be said at present must inevitably not only leave some insistent questions unanswered, but also be very tentative.

Should it ever be shown that electric and magnetic potential gradients are regions of curved space we must expect the kind of curvature to differ basically from the kind that is identified with a gravitational potential gradient, for there is no known interaction between gravitational fields and the other two kinds. A change of the charge on a body having inert mass does not change the behaviour of the body in a gravitational field. A change in the potential gradient of a gravitational field does not affect the forces between electric charges placed in it. Gravitational masses that do not carry electric charges and are not magnetically polarised fall with the same acceleration through an electric field (provided there is a gravitational one to fall in) irrespective of its intensity. In other words, electric charges behave in the same way in gravitationally curved as in flat space.

From such observations it has to be concluded that, if electromagnetic potential gradients are, like gravitational ones, curved regions of space the geometry by which the curvature can be defined can hardly be of the same Riemannian kind that represents gravitational potential gradients. This independence from each other of the different kinds of field takes some accounting for and even leaves room for doubt whether the electro-magnetic field is, as seems so reasonable to suppose, a region where the geometry is non-Euclidean. But I have nothing to contribute to the search for an explanation and do not propose here to discuss the relation between space and charge or that between space and magnetism, but only the relation between space and mass. I have no choice but to discuss this relation as though the only condition of space by which one region is distinguished from another is curvature and the only known kind of curvature is that identified with the gravitational field. The incompleteness of such treatment is made obvious by the known facts about electricity and magnetism. But all that one can do in the present state of ignorance is to see along what path the incomplete treatment takes one and to hope that better knowledge of the relation between space and charge will not necessitate too great a change of direction from that in which the chosen path leads.

I want here to define the limits to our present knowledge of the relation between space and mass. Relativity theory has contributed a great deal to that knowledge. But there is still much about which we are ignorant and questions to which the search for answers is likely to prove rewarding come under two headings: the action of space on mass and the action of mass on space.

Let me now turn to the action of space on mass and remind you of some well-known facts about it.

Relativity theory tells us something about the motion of a particle to which no force is applied, it being well understood that all statements about the motion are relative to a given co-ordinate system. The motion depends, according to the theory, only on the curvature of the space in which the particle finds itself. If the space is flat, the particle moves with a constant velocity, which may of course be zero velocity. If the space is curved, the particle moves with a non-uniform velocity; it experiences an acceleration or a deceleration.

If a force is applied to the particle its movement is no longer wholly determined by the geometry of space. A billiard cue can accelerate a billiard ball in flat space and a shelf can, by exercising a force on the stone resting on it, prevent the stone from following the curvature of the space in which it finds itself.

I mention these well-known facts only to bring out what is and what is not known about the action of space on mass. Relativity theory defines the property of *space* by virtue of which it is able to act on mass; this property is technically called curvature. But relativity theory does not tell us anything about the property of *mass* by virtue of which an unrestrained particle follows the curvature. The property is given the name inertia, but to name a thing is not to explain it or to give any sort of information about it. Relativity has some important things to say about space but fewer about mass. Let me illustrate our present ignorance about the relation between these two concepts with the help of an analogy.

When one sees a tramcar turn a corner, one may give the perfectly true explanation that there are rails for it to travel on. But it is an insufficient explanation. It says something about the street, but nothing about the tram. To make the explanation complete one has to add that the tramcar is provided with flanged wheels, so designed that they fit the rails.

Similarly, when one sees a stone fall one may give the explanation that the space in which the stone happens to be is curved and thereby causes a curved track in space-time. One has then found the equivalent of the rails that take the tram round a corner. The analogy is admittedly not perfect, for a force is exerted between the tramrails and the flanges on the wheels, whereas the stone follows the curvature of space without any force being exerted on it at all. But the analogy serves, nevertheless, to show in what way the relativistic explanation remains incomplete. It fails to include the equivalent of the tram-wheels. What, one is led to ask on receiving the relativistic explanation, is the feature of a particle that causes it to ' engage ' with space, as it were, so that its track in space-time follows the curves? In the example of the tramcar, steel wheels run on steel rails. When an unrestrained particle moves in space something is said by relativists to run on curvature. What is it?

I shall return to the question later. For the moment I want to say a few words about the action of mass on space.

For the sake of convenience I shall again repeat a few well-known facts. According to Newtonian mechanics a massive body causes other bodies in its vicinity to be accelerated. To the question why this happens the answer is that a field of force surrounds the body. But to the question: what sort of a thing is this field? Newtonian mechanics has no answer; it can at most provide a name. Newton did not attempt to explain gravitation; he was content to postulate it.

It is here that relativity theory steps in. It does say what sort of a thing a gravitational field is, namely a region of curved space. The answer is justified both because it is methodologically sound and because it has considerable explanatory power. But it leads to the further question: why should a massive body cause the space around it to be curved? Again, relativity says something about space, but nothing about mass. It is content to postulate the effect of mass on space without explaining it. It seems to me that this limitation of relativity theory has not been properly appreciated. I say advisedly ' limitation ' and not ' defect ' for it is no defect of a theory to fail to provide ' all the answers '.

If this particular limitation has not been much noticed and if little effort has been made during recent decades to get past it, the reason is I think in a deflection of interest. In the early days of relativity interest was directed strongly towards the interaction between particles and, space. Studies in nuclear physics have since then directed more attention towards the interaction between particles and other particles, as already mentioned. Both kinds of interaction deserve equal study and I venture to suggest that a return to the earlier interest would now prove rewarding.

When one turns one's attention to this earlier interest, one is led to remember a view expressed by some relativists of those times. An elementary particle, it was sometimes said, is, apart from its charge, a region of gravitationally curved space and nothing else.

Eddington made this point a fundamental one. ' Mass is curvature ' he said somewhere. At the time when he was writing there was the catch-phrase, ' Man is a kink in space-time '. Pre-occupation with this hypothesis is less insistent today, but not because we are any nearer either to accepting or discarding it. It is only for the reason already mentioned, namely that we are today more concerned with the way particle acts on particle than with the way space acts on particle. The great attention paid to nuclear physics has diverted attention from the fundamentals of relativity. But I do not think that what is at most a slight lack of topicality makes the question whether mass is curvature or something else any less rewarding now than it has been in the past.

To avoid the complication of electricity and magnetism I shall begin by considering only a neutron. What is it made of? Must we postulate a substance that is different in nature from the technical concept, space? Shall we find ourselves obliged to speak of ' particle stuff ' or shall we say, with Eddington, that the neutron consists of curvature and nothing else? When the question is put with this disconcerting candour one is inclined to dislike every answer that can be suggested. But I think that one will have the greatest dislike for the suggestion that there is something deserving of such a title as ' particle stuff '. One will be inclined to accept the Eddingtonian answer, if only as the lesser evil. The theory that the neutron consists of curvature seems btteer to meet the Principle of Minimum Assumption and to offer a better prospect of further unification of physics.

If this is accepted, the volume occupied by a neutron is a region of bound curvature. Within this region the potential gradient does not change with time in the way it does when the curvature takes the form of a travelling wave.

I have said a little while ago ' In the example of the tramcar, steel wheels run on steel rails. When an unrestrained particle moves in space, something is said by relativists to run on curvature. What is it? ' If mass is curvature, the answer is found. Only curvature can run on curvature. It seems as reasonable a conclusion as one may hope for so long as one accepts general relativity.

With that hint I should like now to leave discussion of the features by which one part of a relativistic space is distinguished from another and to turn to the notion of expanding space.

Expanding Space

The theory that space is expanding is supported by two kinds of evidence, that of inference and that of observation.

The inferential evidence was provided first, and by relativists. They explained that a cosmological model of which the volume did not change with time would be unstable. In the sense in which they used the word 'unstable', a model that resembled actuality would have to change its volume. This did not prove that space expands. The inference would have been equally compatible with a model that contracted, which it could, of course, not have been doing for an indefinitely long time without having disappeared. Hence the conclusion that space was, in fact, expanding was first arrived at by reasoning alone.

The observational evidence for the same conclusion is well-known. It is provided by the red shift in the spectrum of the light from distant nebulae. This shift is interpreted as a Doppler effect and is attributed to a recession of the nebulae from each other. The magnitude of the shift is found to a close approximation to be proportional to the distance of the nebulae. It is the effect that was predicted by the inferential evidence.

When a theory has such two-fold support it is usually accepted by scientists. If the notion of expanding space has not gained universal support, the reason is not far to seek. The notion is difficult to understand; one cannot form a picture in one's imagination of space itself expanding. It is only natural to dislike a conclusion that one cannot fully understand. From dislike to rejection is but a short step, so it is not surprising that some rather desperate attempts have been made to find an alternative explanation of the red shift. It has been done, of course, in the name of scientific caution. But the degree of scientific caution with which a new idea is greeted is some indication of its unattractiveness.

But unless both the inferential and the observational evidence can be effectively shaken, the wisest course is to do one's best to come to terms with the notion of expanding space whether one finds it an easy concept or not. If we cannot hope to relate this notion to anything with which we are familiar, we should at least try to find some valid statements about it.

One sometimes says that a fugitive from justice puts space between himself and his pursuers. One does not mean the expression to be taken literally. One only means that the fugitive is running faster than those in pursuit. When he does this he does not create new space but only causes a larger amount of existing space to separate him from the pursuers.

If the fugitive could literally put space between himself and his pursuers, he would not need to run away from them. He could sit down and smoke a cigarette while he put enough space in front of those who were trying to catch him to make sure that they never got any nearer. If he did this he would not be moving past objects in existing space. He would not be moving at all.

It is in this literal sense that, in an expanding universe, space originates between us and every distant nebula. While the fugitive from justice is getting further from his pursuers, he is also getting nearer to the house in which he hopes to hide. But while our galaxy is being caused by the expansion of space to get further from all other galaxies, it is not being caused to get nearer to anything.

In this there is a significant difference between changes that result from the operation of forces between bodies and those that result from the expansion of space. So long as there are forces, some things get nearer to others; they overtake other things. But when space-expansion alone determines distances nothing ever gets nearer to anything else; there is no overtaking; there is not even movement.

It is this last conclusion that makes the notion so difficult to understand. If things get further apart they must, we are inclined to reason, move relatively to each other. But we have to appreciate that this is false reasoning. Let me show why as clearly as possible.

Two nebulae, *A* and *B*, have been observed and both show the red shift. One of them, *A*, is in the part of the sky called North and *B* is in exactly the opposite direction, the part called South. When we are thinking only of *A*, we may make one of two statements:

(1) The distance between our galaxy and the nebula is increasing.
(2) Our galaxy and the nebula are moving relatively to each other.

We may be inclined to think that these two statements have identical meanings; and so they would in many contexts. But if we attribute the red shift to the expansion of space we have to conclude that they mean different things and that, while (1) is correct, (2) is wrong.

This emerges when the implications of (2) are examined. To say that our galaxy and the nebula *A* are moving relatively to each other may mean that both are moving or that one is at rest while the other is moving. But it must mean that at least one of the two bodies is moving.

If this were our own galaxy, it would be moving away from *A*, i.e. southwards. But when we observe nebula *B* we have to conclude that, if our galaxy moves at all, it must be away from *B* and northwards. A corresponding conclusion would be reached if we used a nebula in any other part of the sky as an indication of the direction in which our galazy was moving. Wherever our choice fell, it would always cause us to say that we were moving away from the observed nebula. To say that the expansion of space is causing our galaxy to move is to say that the movement is in all directions at once! The correct interpretation of the red shift is, in other words, that our galaxy is not moving at all relative to any other nebula.

Are we then to take the view that we alone are at rest and that nebulae *A* and *B*, together with all others, are moving relatively to us? Are we to adopt the old egocentric universe in which we are located at a centre from which all effects radiate?

This, we know, cannot be. An observer on any other nebula would have the same experience as we ourselves here. It would be just as impossible for him to state the direction in which his nebula was moving. He could not say that it was moving relative to space in such a way that space, at one moment in front of it, was behind it at the next moment. He would say that his nebula was not overtaking anything, not even empty space; that it was not moving in any direction; that it was at rest.

We are thus obliged, whether we like it or not, to accept the odd notion that in expanding space the distance increases between objects that are all at rest relative to their surroundings. If the nebulae all seem to drift away from each other, this cannot be attributed to anything that happens to the nebulae but only to what happens to the space between them.

Even if one succeeds in appreciating the strange fact that, in expanding space, objects remain stationary while the distance between them increases, one may still, I fear, hanker after a force that drives them away from each other. It seems to the way of thinking of most of us axiomatic that things can only get further apart if a force is pushing them away from each other. But a little very simple mathematics shows that the notion of forces of repulsion has to be given up, however reluctant one may be to do so.

The velocity of recession, as viewed in any one particular direction, is:

$$dl/dt = Hl$$

where l is distance and H is Hubble's constant. To find its value observations are necessary that cannot be made with very great precision. Recently it was thought to be 185 km/sec/megaparsec. But later observations suggest that 100 km/sec/megaparsec may be nearer to the truth. If H is a cosmological constant and holds for the whole of space-time the acceleration is:

$$A = d_2l/dt^2 = Hdl/dt = H^2l \quad . \quad . \quad (1)$$

Here A is the acceleration of one body relative to a single selected other one, but not the acceleration of any body relative to all other ones, which would always be zero.

Force is the product of mass and acceleration. If our galaxy were receding under the influence of a repelling force, we should therefore give this the value

$$F = mA = mH^2l \quad . \quad . \quad . \quad . \quad (2)$$

where m was the mass of our receding galaxy. But this would be an absurd conclusion. According to equation (2), the force exerted by a body on any other one would not be proportional to the mass of the repelling body but to that of the repelled one.

Such a conclusion cannot, of course, be reconciled with the known law of gravitational attraction. Consider two bodies with the respective masses m_1 and m_2. The gravitational force between them, which needs the negative sign as it is one of attraction, is:

$$F = -Gm_1m_2/l^2.$$

Here the force exerted by m_1 on m_2 is the same as that exerted by m_2 on m_1. But if equation (2) meant anything (which it does not), one would have to express the force exerted by m_1 on m_2 as m_2H^2l and that exerted by m_2 on m_1 as m_1H^2l. The tiniest repelling mass m_1 would exert a very big force on m_2 if this were big.

Let the distinction between a change of distance that is due to expanding space and one that is due to a force be expressed in a slightly different way. The acceleration of mass m_2 relative to m_1 is $-\ Gm_2/l^2$, and the acceleration of m_1 relative to m_1 is $-\ Gm_1/l^2$. The total acceleration of the two masses relative to each other in expanding space is:

$$A \text{ total} = kH^2l - G(m_1 + m_2)/l^2 \quad . \quad . \quad . \quad (3)$$

This expression shows clearly that the relative acceleration occasioned by gravity is dependent on both masses as well as on the distance between them, while the relative acceleration occasioned by the expansion of space is dependent only on the distance between the masses. It is a function of space and of nothing else. The conclusion is disconcerting. But efforts to avoid it have to be rather desperate and to introduce sundry *ad hoc* hypotheses. They are not sought so much by those who accept the relativistic view of space as by those who reject or have forgotten it.

The proper conclusion is thus, that the distance between things can increase while the things are at rest relatively to each other. It is the space between them that originates. The notion of expanding space is more aptly called the Hypothesis of the Continuous Origin of Space.

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FROM DUALISM TO UNITY IN QUANTUM MECHANICS*

ALFRED LANDÉ

1 *Idols of Duality*

THE doctrine of duality maintains that neither discrete particles nor continuous waves have an exclusive claim to physical reality, that both are rationalised pictures fitting different experimental situations which, being complementary, yield a more complete knowledge of a microphysical object only in their combination. I concede that a neutral stand toward the wave-particle controversy, leaning on the doctrine of duality, was justified during the earlier period of quantum theory when the evidence pointed sometimes in this, sometimes in the opposite direction. But I maintain that the continued cult of duality and complementarity represented as *fundamental* features of the microphysical world has become an all too convenient evasion of a serious proproblem of theoretical physics, viz. that of establishing a unitary theory of matter resting on a clear decision between particles or waves as the *real* constituents of matter. Realism is the only stand which can satisfy physicists in the long run (Einstein certainly was a physicist). Dialectical positivism practised by Bohr and Heisenberg may serve only as a temporary stopgap, as a comfortable excuse for indecision resting on an unfilled gap in our knowledge of theoretical interconnections between data. Actually, a decisive step in the direction of unitary theory was taken already thirty years ago by Max Born. According to his statistical interpretation, wave-like *appearances* are the combined effects of statistical arrangements of *real* particles ; this was a clear repudiation of dualism with its claim that particles and waves are mere ' pictures ' and of equal rank. Those who accept Born's statistical particle interpretation cannot speak of duality any more than they would speak of the duality of water droplets versus water waves with autonomous wave laws, (*a*) because it is illogical to construe an opposition between things (droplets) and one of their many qualities

* Received 4. xi. 58

(their occasional arrangement as waves), and (*b*) because the apparently autonomous wave laws can be reduced to general mechanical principles for droplets as the real basic constituents of water. In quantum theory, however, anything goes. The same theorists who adhere to Born's realistic particle interpretation of wave-like appearances during their working days, pay lip service to an alleged 'fundamental duality' in their Sunday talk. Thereby they not only violate the simple logical rule (*a*); but they ignore that, similar to (*b*), the appearance of separate wave laws in quantum mechanics can be reduced to a combination of general elementary principles for the statistical behaviour of particles—principles which do not presuppose anything wave-like, as will be discussed in Section 3 below. (We here deal exclusively with elementary quantum mechanics, disregarding relativistic theories of creation and annihilation of matter in real measurable fields, in contrast to the waves of elementary quantum mechanics which for adherents of Born's unitary particle interpretation, i.e. for non-dualists, are but 'expectation waves' for particles.) The philosophically adorned vacillation between workday particle realism and Sunday dualistic positivism may be excused, however, by the fact that Born's particle *interpretation* has never been developed into a complete theory by furnishing an *explanation* as to why particles, in their statistical manifestations, should obey interference and other wave-like laws. By 'explanation' we here mean a reduction of the puzzling wave phenomena displayed by particles to simple and unsophisticated general principles valid for particles, without resorting to wave arguments, i.e. without taking the explicandum for granted—as does the Copenhagen doctrine when it answers the question 'why' above with: 'don't you know that duality and complementarity are *fundamental* qualities of matter?' It is a similar evasion of the basic quantum problem when the physicist Dyson[1] recently reports with obvious satisfaction :

> The student begins by learning the tricks of his trade . . . then he begins to worry because he does not understand what he has been doing. This stage often lasts six months or longer, and it is strenuous and unpleasant. Then quite unexpectedly . . . the student says to himself: 'I understand quantum mechanics', or rather he says: 'I understand now that there isn't anything to be understood.' . . . For each new generation of students there is less resistance to be broken down before they feel at home with quantum ideas. . . .

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[1] F. J. Dyson, *Scientific American*, Sept., 1958

I must confess that for myself, and I am certain also for a number of other physicists, the state of worry has lasted many years rather than six months; they and I still refuse to be ' broken down ' to accepting rules without an ' understanding ' of them from a unitary point of view.

It is always helpful to look into the history of a disputed ideology. In contrast to the well-established atomic theory of matter, the phenomena of interference discovered by Davisson-Germer and G. P. Thomson seemed to indicate a continuous substratum supporting matter waves, a hypothesis strongly supported by the immense success of Schrödinger's wave equation. Theory was thus faced with the following problem: Supposing that matter really consists of particles, how can one explain, from the pure particle point of view and with the least amount of *ad hoc* hypotheses, the appearance of wave-like phenomena? And *vice versa*, suppose matter is continuous, how can one explain individual clicks in Geiger counters and other phenomena indicating particles with definite measurable masses and charges? It was assumed of course that only one of the two constitution hypotheses could be accepted in the long run because ' you cannot have peasoup and whole peas at the same time ' (Einstein).

2 *Ways of Escape*

A solution of the dilemma seemed to arise from Schrödinger's result that, according to his wave equation, the *crests* formed by a group of material waves move along exactly as particles would move according to the laws of ordinary mechanics. Hence, what *appears* to be a particle might *in reality* be the crest of a wave group. This would indeed offer a most fascinating explanation of corpuscular appearances within *unitary wave theory*. Unfortunately, just according to the wave equation, the steeper a crest maximum, the faster will it flatten out and thereby lose all resemblance to a discrete particle. The attempt at establishing a unitary wave theory of continuous matter thus failed; it could not account for particle-like phenomena.

To end this deadlock, Max Born took the opposite stand. What *appears* to be a wave intensity is *in reality* the density of a statistical or probability distribution of particles. And Schrödinger's wave crest motion indicates that ordinary mechanics holds in the statistical average, though not for individual particles, a most satisfactory result connecting micro- with macromechanics. But neither Born nor his successors deemed it necessary to provide an *explanation*, resting on pure corpuscular arguments, why particles should ever display a wave-like interference and other wave features in their statistical behaviour. Don't we have the Schrödinger wave equation? That is ' why '! *Unitary particle theory* so auspiciously begun by Born thus remained a torso.

Instead of regarding the lack of explanation of wave phenomena as a challenge to further efforts at constructing a complete quantum theory of particles, most physicists yielded to the nordic siren song—and among the Greats only Einstein, de Broglie, and Schrödinger ignored it—of *talking away* a problem by philosophical reflection, rather than solving it by the concrete methods of theoretical physics. If a problem seems to resist all efforts of solution, it may be a ' pseudo-problem '. At any rate it deserves a proper name such as ' fundamental duality ' and ' universal complementarity '. Moreover, why should one try to ' explain ', or to decide between two rival ' realities '! The belief in a reality behind the phenomena, independent of thought and sensation, is a metaphysical prejudice. And there are no eternal and self-evident truths through which nature could be ' explained '. Let the physicists look for economical description; and if they find two economical descriptions, let us guard against bias, in particular when the two turn out to be complementary. Duality must be regarded as ' fundamental ', as an immanent characteristic of all matter. The desire for a unitary realistic theory is old-fashioned and naïve.

To all this I reply: your dialectical positivism and neutrality is pure opportunism, is an evasion of a clearcut problem of theoretical physics, is turning inability into a virtue. If unitary *wave* theory collapsed together with the wave crests, there is no reason to believe that unitary *particle* theory, too, should be doomed to failure for all time. Nor is it justified to put the physical question whether matter really is continuous or discrete, on a level with the deep philosophical issue of reality which has occupied thinkers from Locke and Berkeley to modern times. Physics starts from the position of naïve realism. Margenau's firefly[1] continues as one and the same real being also between visible flashes at night. In ' Quantentheorie und Wirklichkeit ' Einstein [2] replies to the subjectivistic positivism of Bohr and Heisenberg as follows: ' The concepts of physics refer to a real world, to things that claim real existence, independent of perceiving subjects.' And when further discussing his view he speaks, as would anybody else, of electronic *particles*, and he wonders how and why these real particles display wave-like phenomena, or *vice versa.* To Einstein and to other realists, i.e. to all physicists on weekdays, *whole peas or peasoup is still the question.* It is not a ' pseudo-problem ' which can be disputed away. The unqualified answer rather is ' peas '; it will be developed in the next paragraph.

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[1] H. Margenau, *The Nature of Physical Reality*, New York, 1950

[2] A. Einstein, *Dialectica*, 1948, **2**, 320

3 *Unitary Particle Theory*

Destructive criticism is of little value unless followed by positive suggestion for improvement. The following short outline may indicate how a corpuscular explanation of the wave-like laws can be obtained within the statistical particle interpretation.

(*A*) The most prominent difference of quantum from classical mechanics is the occurrence of statistically ruled events, viz. transitions of a micro-mechanical system from one state to another during its interaction with a macroscopic instrument of measurement. The author has shown on other occasions[1] that statistical distribution of effects issuing from one and the same cause are closely related to, indeed must occur under, the postulate of *cause-effect continuity*, which requires a graded scale of fractional equality values between two states A and B of the same mechanical system (particle), ranging from $A = B$ (identity, inseparability) to $A \neq B$ (complete inequality, separability of A and B) *via* intermediate cases $A \sim B$ of ' fractional equality ' or fractional separability. The latter case implies that a ' filter ' built for *passing* particles in the state A with certainty, will react to B-state particles when $B \sim A$ by sometimes passing, sometimes rejecting them, at a statistical ratio which may serve as a quantitative measure of the ' equality fraction ' between A and B, denoted as $P(A, B)$. The same fraction $P(A, B)$ also indicates the probability of transition from the state B to A and *vice versa.* The postulate of cause-effect continuity thus yields *sufficient reasons for the lack of sufficient causes* for individual events.

(*B*) The statistical particle *interpretation* calls for an *explanation* of two characteristic wave-like features of particle behaviour, one being the *interference* law of probability amplitudes. Instead of accepting this wave-like law for particles at face value, or as a manifestation of a universal ' principle ' of duality, it may be explained as follows.[2] For very general reasons (symmetry of the probabilities of transition both ways in correspondence with classical reversibility) one can arrange the probabilities P in quadratic tables in which every row and every column sums up to *unity*. Each table (P_{AB}) which connects one set of states A (e.g. the states of energy) with another set B (e.g. the states of position) is a ' unit magic square matrix '. *A priori* one might think that the various P-tables, (P_{AB}), (P_{BC}), $P(_{AC})$, etc. are entirely independent of one another. If, however, one introduces the postulate that the various P-tables are interconnected by a general self-consistent law at all, rather than presenting a chaos, then one arrives by pure mathematical reasoning at the conclusion that this P-connection law can *only* be of the form familiar to mathematicians as *unitary transformation.* The latter, however, is known to physicists as the *interference* law of probabilities. In detail: unitary transformation associates with every element P of a unit magic square matrix a complex quantity ψ, i.e. a vector in a plane giving direction to the quantity P. The vectors ψ then represent a structural framework in a plane by virtue of the matrix multiplication law, $(\psi_{AC}) = (\psi_{AB}) \cdot (\psi_{BC})$, which is identical with the interference law of quantum physics. The simple postulate that *a general P-connection law exists at all* leads by mathematical necessity to interference, so that this wave-like feature is explained on a purely corpuscular statistical basis, without appealing to ' duality '.

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[1] A. Landé, this *Journal*, 1956, **6**, 300; *Mind*, 1958, **67**, 174; *Philos. Sci.*, 1957, **24**, 309

[2] A. Landé, *Phys. Rev.*, 1957, **108**, 891

(*C*) Another quantum feature, usually introduced *ad hoc* or with reference to wave-particle duality, is the *periodic* connection between co-ordinates q and momenta p, as well as between energy and time, best known through Planck's rule $E = h\nu$ and de Broglie's rule $p = h/\lambda$. This periodic relation between ' conjugate ' dynamical observables, which re-appears in modern quantum mechanics as Born's commutation rule, $qp - pq = h/2i\pi$, and as Schrödinger's p-operator rule, is the backbone of quantum dynamics. It usually is accepted as fundamental without further attempt at understanding it from the particle point of view. To those not yet ' broken down ' to accepting quantum theory at face value, it is the most intriguing quantum riddle. Yet it may be explained on the basis of particle mechanics alone, without appealing to wave-particle duality, in the following manner.[1]

' Conjugate observables ' such as q and p, have a statistical density distribution which is *constant* for all q-values at given p, and *constant* for all p-values at given q. This constant statistical density in q- and in p-space is well known from classical theory; it is not a separate quantum assumption; nor does it imply any *periodic* relation between

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[1] A. Landé, *Am. J. Phys.*, 1956, **24**, 56; *Foundations of Quantum Theory : a Study in Continuity and Symmetry*, New Haven, 1955

q and p. Yet, in combination with the above-mentioned general ψ-interference law, it leads by mathematical necessity to the result that the probability amplitude function $\psi(q, p)$ must be of the complex exponential form, $\exp(2i\pi qp/\text{const})$, describing a ' wave function ' of wave length $\lambda = \text{const}/p$. With the constant denoted by the familiar letter h one thus arrives at the most prominent feature of the quantum mechanics of particles, the *periodic* connection between co-ordinates and momenta, without appealing to wave arguments of duality.

4 *General Conclusions*

How does all this affect the status of duality, qp-periodicity, and complementarity as fundamental and ultimate principles of nature? Let us ask the following analogous question: Someone has found experimentally that in every paper-cut triangle, the three altitudes intersect in one point, and that the same applies to the three medians and the three angular bisectors. He concludes that he has discovered a deep and fundamental geometrical feature, the principle of unity in trinity, with analogies in social relations, music, and so forth. Later he realises that his discovery could have been derived as a consequence of the (almost self-evident) general axioms of Euclid. Will he still maintain that ' unity in trinity ' is an ultimate principle? The writer does not question the usefulness of the wave-particle antithesis for heuristic purposes. But he thinks that a better understanding of quantum mechanics can be attained—for those who did not yet arrive at the experts' view that ' there isn't anything to be understood '—by recognising the quantum principles as consequences of more general, more elementary, ground postulates upon which one can erect a *unitary particle theory* which at the same time *explains* wave-like appearances rather than merely accepting them as matters of fact or as manifestations of an immanent ' duality '.

In a recent article [1] Niels Bohr reiterates what he has told us many times before:

> This difficult task (of a rational generalization of classical mechanics) was eventually accomplished by the introduction of appropriate mathematical abstractions. Thus in the quantum formalism, the quantities by which the state of a physical system is ordinarily defined, are replaced by symbolic operators subjected to a non-commutative algorism involving Planck's constant.

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[1] Niels Bohr, ' Quantum Mechanics and Philosophy ', *Survey of Philosophy in Mid-Century*, Copenhagen, 1958

However, the same might be said of plain ordinary wave optics where it also has been found convenient to use the cabbalistic sign $\sqrt{-1}$ as an 'appropriate mathematical abstraction'. In my opinion it is time to overcome the idea that quantum mechanics is a kind of mathematical sorcery conjuring observable results as materialisations of abstract laws of nature which 'cannot be understood'. The simple laws of quantum mechanics rather are rational consequences of ordinary, elementary, plausible, and general principles concerning the statistical behaviour of matter particles, which can be grasped by any person who is ready to abandon the obsolete cult of dualism in favour of a unitary theory in which *real* particles produce wave-like *appearances*, as proposed thirty years ago by Max Born.

A few remarks may be added concerning the *Theory of Measurement* which has come into the limelight again through the Bristol Symposium on *Observation and Interpretation* of 1957. The starting point of the discussion always is the Heisenberg-von Neumann story of the sudden contraction and gradual expansion of a wave packet, telling us that a mechanical system undergoes two kinds of process, namely (*a*) a gradual change of state controlled by the reversible Schrödinger equation when the system is left alone, and (*b*) a sudden irreversible change of state during an act of measurement. Physicists are wondering of course how a consistent theory could account for two such entirely different kinds of process. My answer is that the tale of the two processes is a myth resting on a double meaning of the term 'state'. In (*a*) state means 'state of expectation' in the mind of some observer who knows quantum mechanics. In (*b*) it means 'physical state' of the system.[1] A sudden change of word-meaning cannot of course be understood in terms of a differential equation.

The source of the double meaning can be seen in a wrong interpretation of the Schrödinger equation as a 'process equation', as describing the gradual change of the physical state of a system. The Schrödinger equation does nothing of the kind (if one accepts Born's statistical interpretation). It rather describes a mathematical relation between various formalistic representations of one and the same physical state S [or Hilbert vector S, represented in terms of its orthogonal components with respect to the states $(xyz,\ t_A)$, and also represented by its orthogonal components with respect to the states

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[1] A. Landé, 'Zur Quantentheorie der Messung', *Zeitschr. Physik*, 1958, **153**, 389

(xyz, t_B)], the component representations being $\psi_s\ (xyz, t_A)$ and $\psi_s\ (xyz, t_B)$, and the two functions ψ_s being connected by the Schrödinger equation.

Summary

When Schrödinger's idea of a unitary wave theory of matter, with wave crests giving the appearance of particles, proved untenable, Born took the first step toward a unitary particle theory of matter. However, his interpretation was never completed into an explanation as to why the statistical behaviour of particles should be wave-like. The temporary lack of a complete unitary particle theory was then turned into a virtue by proclaiming dualism as 'fundamental', thus talking away a problem rather than solving it. The cult of duality and complementarity is an opportunistic evasion of a clear problem of theoretical physics, which can be solved, however. Three steps leading to a solution of the problem of wave appearances within pure particle theory are outlined; details have been published in physical journals. A return from the subjectivistic positivistic neutrality of Bohr and Heisenberg to a realistic unitary theory is possible.

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THE PROPENSITY INTERPRETATION OF PROBABILITY

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I

In this paper I intend to put forward some arguments in favour of what I am going to call the propensity interpretation of probability.

By an interpretation of probability—or, more precisely, of the theory of probability—I mean an interpretation of such statements as,

'The probability of a given b is equal to r'

(where r is a real number); a statement which we can put in symbols as follows:

$$`p(a, b) = r.'$$

There have been many interpretations of these probability statements, and years ago I have divided these interpretations into two main classes—the subjective and the objective interpretations.[1]

The various subjective interpretations have all one thing in common: probability theory is regarded as a means of dealing with the *incompleteness of our knowledge*, and the number $p(a, b)$ is regarded as a measure of the degree of rational assurance, or of rational belief, which the knowledge of the information b confers upon a (a is in this context often called 'the hypothesis a').

The objective interpretations may also be characterised by a common feature: they all interpret

$$p(a, b) = r$$

as a statement that can, in principle, be objectively *tested*, by means of statistical tests. These tests consist in sequences of experiments: b in $p(a, b) = r$, describes the experimental conditions; a describes some of the possible *outcomes* of the experiments; and the number r describes the *relative frequency* with which the outcome a is estimated to occur in any sufficiently long sequence of experiments characterised by the experimental conditions b.

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[1] See my *Logic of Scientific Discovery* (1934, 1959), section 48, and appendix* ii

It is my conviction that most of the usual applications of subjective interpretation of probability are untenable. There may be something like a measurable degree of the rationality of a belief in *a*, given the information *b*; but I assert that this belief cannot be adequately measured by a measure that satisfies the laws of the calculus of probability.[1] (I think it likely, however, that 'degree of confirmation or corroboration'—the latter term is preferable—will turn out to furnish, under certain circumstances, an adequate measure of the rationality of a belief; see my notes on 'Degree of Confirmation', this *Journal*, 1954, **5**, 143, 334, 359; 1957, **7**, 350, and 1958, **8**, 294.)

As to the objective interpretations, the simplest seems to be *the purely statistical or the frequency interpretation.* (I take these two designations as synonymous.) This interpretation regards the statement

$$p(a, b) = r$$

as an estimate, or a hypothesis, asserting *nothing but* that the relative frequency of the event *a* in a sequence defined by the conditions *b* is equal to *r*. Or in other words, the statement '$p(a, b) = r$' is interpreted to mean: 'events of the kind *a* occur, in sequences characterised by *b*, with the frequency *r*'. Thus, for example, '$p(a, b) = \frac{1}{2}$' may mean 'the relative frequency of tossing heads with a normal penny equals $\frac{1}{2}$' (where *a* is getting heads upmost, and *b* is a sequence of tosses with a normal penny).

The frequency interpretation has been often criticised, but I believe that it is possible to construct a frequency theory of probability that avoids all the objections which have been raised and discussed. I have sketched such a theory many years ago (it was a modification of the

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[1] The most characteristic laws of the calculus of probability are (1) the addition theorems, pertaining to the probability of $a \vee b$ (that is, of *a*-or-*b*); (2) the multiplication theorems, pertaining to the probability of *ab* (that is, of *a*-and-*b*); and (3) the complementation theorems, pertaining to the probability of $\bar{a}$ (that is, of non-*a*). They may be written

$$(1)\quad p(a \vee b, c) = p(a, c) + p(b, c) - p(ab, c)$$

$$(2)\quad p(ab, c) = p(a, bc)p(b, c)$$

$$(3)\quad p(\bar{a}, c) = 1 - p(a, c), \text{ provided } p(\bar{c}, c) \neq 1.$$

The form of (3) here given is somewhat unusual: it is characteristic of a probability theory in which

$$(4)\quad p(a, c\bar{c}) = 1$$

is a theorem. The first axiom system for a theory of this kind was presented, as far as I know, in this *Journal*, 1955, **6**, 56. See also my *Logic of Scientific Discovery*, appendix* iv, and the appendix to the present paper.

theory of Richard von Mises), and I still believe that (after some minor improvements which I have made since) it is immune to the usual objections. Thus the reason why I changed my mind in favour of the propensity interpretation was not that I felt I had to give way to these objections (as has been suggested by W. C. Kneale in a discussion of a paper of mine [1]). Rather, I gave up the frequency interpretation of probability in 1953 for two reasons.

(1) The first was connected with the problem of the interpretation of quantum theory.

(2) The second was that I found certain flaws in my own treatment of the probability of *single events* (in contrast to sequences of events), or ' singular events ', as I shall call them in analogy to ' singular statements '.

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Although the bulk of the present paper is devoted to a discussion of the second of these two points, I wish to mention first very briefly the reasons connected with the first point, because it was the first point in time and importance: it was only after I had developed, and tried out, the idea that probabilities are *physical propensities*, comparable to Newtonian forces, that I discovered the flaw in my treatment of the probability of singular events.

I had always been convinced that the problem of the interpretation of the quantum theory was closely linked with the problem of the interpretation of probability theory in general, and that the Bohr-Heisenberg interpretation was the result of a subjectivist interpretation of probability. My early attempts to base the interpretation of quantum theory upon an objective interpretation of probability (it was the frequency interpretation) had led me to the following results.

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[1] W. C. Kneale said in this discussion: ' More recently the difficulties of the frequency interpretation, i.e. the muddles, if not the plain contradictions, which can be found in von Mises, have become well known, and I suppose that these are the considerations which have led Professor Popper to abandon that interpretation of probability.' See *Observation and Interpretation*, edited by S. Körner, 1957, p. 80. I am not aware of any ' muddles ' or ' contradictions ' in the frequency theory which have become well known more recently; on the contrary, I believe that I have discussed all objections of any importance in my *Logic of Scientific Discovery* when it was first published in 1934, and I do not think that Kneale's criticism of the frequency theory in his *Probability and Induction*, 1949, presents a correct picture of the situation prevailing at any time since 1934. One objection of Kneale's (see especially p. 156 of his book) was not discussed in my book—that, in the frequency theory, a probability equal to one does not mean that the event in question will occur without exception (or ' with certainty '). But this objection is invalid; it can be shown that every adequate theory of probability (if applicable to infinite sets) must lead to the same result.

(1) The so-called ' problem of the reduction of the wave packet ' turns out to be a problem inherent in every probabilistic theory, and creates no special difficulty.

(2) Heisenberg's so-called indeterminacy relations must not be interpreted subjectively, as asserting something about our possible knowledge, or lack of knowledge, but objectively, as scatter-relations. (This removes an asymmetry between p and q which is inherent in Heisenberg's interpretation unless we link it with a phenomenalist or positivist philosophy; see my *Logic of Scientific Discovery*, p. 451.)

(3) The particles have paths, i.e. momentum and positions, although we cannot predict these, owing to the scatter relations.

(4) This was also the result of the imaginary experiment (' thought-experiment ') of Einstein, Podolski, and Rosen.

(5) I also produced an explanation of the interference experiments (' two-slit-experiments '), but I later gave this up as unsatisfactory.

It was this last point, the interpretation of the two-slit-experiment, which ultimately led me to the propensity theory: it convinced me that probabilities must be ' physically real '—that they must be physical propensities, abstract relational properties of the physical situation, like Newtonian forces, and ' real ', not only in the sense that they could influence the experimental results, but also in the sense that they could, under certain circumstances (coherence), interfere, i.e. interact, with one another.

Now these propensities turn out to be *propensities to realise singular events.* It is this fact which led me to reconsider the status of singular events within the frequency interpretation of probability. In the course of this reconsideration, I found what I thought to be independent arguments in favour of the propensity interpretation. It is the main purpose of the present paper to present this line of thought.[1]

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The subjective interpretation of probability may *perhaps* be tenable as an interpretation of certain gambling situations—horse racing, for example—in which the objective conditions of the event are ill-defined and irreproducible. (I do not really believe that it is applicable even to situations like these, because I think that a strong case could be made —if it were worth making—for the view that what a gambler, or a ' rational better ', tries to find out, in order to bet upon it, is always and invariably the *objective* propensities, the *objective* odds of the event: thus the man who bets on horses is anxious to get more information about horses—rather than information about his own state of belief, or about the logical force of the total information in his possession.) Yet in the typical game of chance—roulette, say, or dicing, or tossing pennies—and in all physical experiments, the subjective interpretation fails completely. For in all these cases probabilities depend upon the *objective conditions of the experiment.*[1]

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[1] The remaining sections 3 to 5 of this paper follow closely a section of my forthcoming book, *Postscript: After Twenty Years.* See also my paper ' The Propensity Interpretation of the Calculus of Probability and The Quantum Theory ', in *Observation and Interpretation*, edited by S. Körner, 1957.

In the remaining sections of this paper, the discussion will be confined solely to the problem of interpreting the probability of ' *singular events* ' (or ' occurrences '); and it is the frequency theory of the probability of *singular events* which I have in mind whenever I speak here of the frequency interpretation of probability, in contradistinction to the propensity interpretation.

From the point of view of the frequency interpretation, the probability of an *event of a certain kind*—such as obtaining a six with a particular die—can be *nothing but* the relative frequency of this kind of event in an extremely long (perhaps infinite) sequence of events. And if we speak of the probability of a *singular* event such as the probability of obtaining a six in the third throw made after nine o'clock this morning with this die, then, according to the purely statistical interpretation, we mean to say *only* that this third throw may be regarded as a member of a sequence of throws, and that, in its capacity as a member of this sequence, it shares in the probabilities of that sequence. It shares, that is to say, those probabilities which are *nothing but the relative frequencies* within that sequence.

In the present section I propose to argue against this interpretation, and in favour of the propensity interpretation. I propose to proceed as follows. (1) I will first show that, from the point of view of the frequency interpretation, objections must be raised against the propensity interpretation which appear to make the latter unacceptable. (2) I will next give a preliminary reply to these objections; and I will then present, as point (3), a certain difficulty which the frequency interpretation has to face, though it does not, when first raised, look like a serious difficulty. (4) Ultimately I will show that in order to get over this difficulty, the frequency interpretation is forced to adopt a modification which appears to be slight at first sight; yet the adoption of this apparently slight modification turns out to be equivalent to the adoption of the propensity interpretation.

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[1] A criticism of the subjective theory of probability will be found in my notes in this *Journal*, quoted above, and in my paper ' Probability Magic, or Logic out of Ignorance ', *Dialectica*, 1957, 354-374.

(1) From the point of view of a purely statistical interpretation of probability it is clear that the propensity interpretation is unacceptable. For propensities may be explained as possibilities (or as measures or 'weights' of possibilities) which are endowed with tendencies or dispositions to realise themselves, and which are taken to be responsible for the statistical frequencies with which they will in fact realize themselves in long sequences of repetitions of an experiment. Propensities are thus introduced in order to help us to explain, and to predict, the statistical properties of certain sequences; and *this is their sole function.* Thus (the frequency theorist will assert) they do not allow us to predict, or to say, *anything whatever* about a single event, except that its repetition, under the same conditions, will generate a sequence with certain statistical properties. All this shows that the propensity interpretation can add nothing to the frequency interpretation except a new word—'propensity'—and a new image or metaphor which is associated with it—that of a tendency or disposition or urge. But these anthropomorphic or psychological metaphors are even less useful than the old psychological metaphors of 'force' and of 'energy' which became useful physical concepts only to the extent to which they lost their original metaphysical and anthropomorphic meaning.

This, roughly, would be the view of the frequency theorist. In defending the propensity interpretation I am going to make use of two different arguments: a preliminary reply (2), and an argument that amounts to an attempt to turn the tables upon the frequency theorist; this will be discussed under (3) and (4).

(2) As a preliminary reply, I am inclined to accept the suggestion that there is an analogy between the idea of propensities and that of forces—especially fields of forces. But I should point out that although the labels 'force' or 'propensity' may both be psychological or anthropomorphic metaphors, the important analogy between the two ideas does not lie here; it lies, rather, in the fact that both ideas draw attention to *unobservable dispositional properties of the physical world*, and

thus help in the interpretation of physical theory. Herein lies their usefulness. The concept of force—or better still, the concept of a field of forces—introduces a dispositional physical entity, described by certain equations (rather than by metaphors), in order to explain observable accelerations. Similarly, the concept of propensity, or of a field of propensities, introduces a dispositional property of singular physical experimental arrangements—that is to say, of singular physical events—in order to explain observable frequencies in sequences of repetitions of these events. In both cases the introduction of the new idea can be justified only by an appeal to its usefulness for physical theory. Both concepts are ' occult ', in Berkeley's sense, or ' mere words '.[1] But part of the usefulness of these concepts lies precisely in the fact that they suggest that the theory is concerned with the properties of an *unobservable* physical reality and that it is only some of the more superficial effects of this reality which we can observe, and which thus make it possible for us to test the theory. The main argument in favour of the propensity interpretation is to be found in its power to eliminate from quantum theory certain disturbing elements of an irrational and subjectivist character—elements which, I believe, are more ' metaphysical ' than propensities and, moreover, ' metaphysical ' in the bad sense of the word. It is by its success or failure in this field of application that the propensity interpretation will have to be judged.

Having stressed this point I proceed to my main argument in favour of the propensity interpretation. It consists in pointing out certain difficulties which the frequency interpretation must face. We thus come to the point (3), announced above.

(3) Many objections have been raised against the frequency interpretation of probability, especially in connection with the idea of infinite sequences of events, and of limits of relative frequencies. I shall not refer to these objections here because I believe that they can be adequately met. Yet there is a simple and important objection which has not, to my knowledge, been raised in this form before.

Let us assume that we have a loaded die, and that we have satisfied ourselves, after long sequences of experiments, that the probability of getting a six with this loaded die very nearly equals $1/4$. Now consider a sequence b, say, consisting of throws with this loaded die, but including a few throws (two, or perhaps three) with a homogeneous and symmetrical die. Clearly, we shall have to say, with respect to each of these few throws with this correct die, that the probability of a six is 1/6 rather than 1/4, in spite of the fact that these throws are, according to our assumptions, *members of a sequence* of throws with the statistical frequency 1/4.

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[1] See my ' Note on Berkeley as a Precursor of Mach ', this *Journal*, 1953, 4, 21 (4).

I believe that this simple objection is decisive, even though there are various possible rejoinders.

One rejoinder may be mentioned only in passing, since it amounts to an attempt to fall back upon the subjectivist interpretation of probability. It amounts to the assertion that it is our special *knowledge*, the special *information* we have concerning these throws with the correct die, which changes the probability. Since I do not believe, for many reasons, in the subjective theory of probability, I am not inclined to accept this assertion. Moreover, I believe that the case before us even suggests a new argument (although not a very important one) against the subjective theory. For we may not know which of the throws are made with the correct die, although we may know that there are only two or three such throws. In this case it will be quite reasonable to bet (provided we are determined to bet on a considerable number of throws) on the basis of a probability 1/4 (or very close to 1/4), even though we do know that there will be two or three throws on which we should not accept bets on these terms, if only we could identify them. We know that, in the case of these throws, the probability of a six is less than 1/4—that it is, in fact, 1/6; but we also know that we cannot identify these throws, and that their influence must be very small if the number of bets is large. Now it is clear that, as we nevertheless attribute to these unknown throws a probability of 1/6, we do not mean by the word ' probability ', and cannot possibly mean by it, a ' reasonable betting quotient in the light of our total actual knowledge' as the subjective theory has it.

But let us now leave the subjective theory entirely aside. What can the frequency theorist say in reply to our objection ?

Having been a frequency theorist myself for many years, I know fairly well what my reply would have been.

The description given to us of the sequence *b* shows that *b* is composed of throws with a loaded die and of throws with a correct die. We estimate or, rather, we conjecture (on the basis of previous experience, or of intuition—it never matters what is the ' basis ' of a conjecture) that the side six will turn up in a sequence of throws of the loaded die with the frequency 1/4, and in a sequence of throws with the correct die with the frequency 1/6. Let us denote this latter sequence, that of throws with the correct die, by 'c'. Then our information as to the composition of b tells us (i) that $p(a, b) = 1/4$ (or very nearly so), because almost all throws are with the loaded die, and (ii) that bc—that is to say the class of throws belonging to both b and c—is not empty; and since bc consists of throws belonging to c, we are entitled to assert that the singular probability of a six, among those throws which belong to bc, will be $1/6$—by virtue of the fact that these singular throws are members of a sequence c for which we have $p(a, c) = 1/6$.

I think that this would have been my reply, by and large; and I now wonder how I could ever have been satisfied with a reply of this kind, for it now seems plain to me that it is utterly unsatisfactory.

Of course there is no doubt as to the compatibility of the two equations

$$\text{(i)}\quad p(a, b) = 1/4,$$
$$\text{(ii)}\quad p(a, bc) = 1/6,$$

nor is there any question that these two cases can be realised within the frequency theory: we *might* construct some sequence b such that equation (i) is satisfied, while in a selection sequence bc—a very long and virtually infinite sequence whose elements belong both to b and to c—equation (ii) is satisfied. *But our case is not of this kind.* For bc is not, in our case, a virtually infinite sequence. It contains, according to our assumption, at most three elements. In bc the six may come up not at all, or once, or twice, or three times. But it *certainly* will not occur with the frequency $1/6$ in the sequence bc because we *know* that this sequence contains at most three elements.

Thus there are only two infinite, or very long, sequences in our case: the (actual) sequence b and the (virtual) sequence c. The throws in question belong to both of them. And our problem is this. Although they belong to both of these sequences, and although we only know that these particular throws bc occur somewhere in b (we are not told where, and we are therefore not able to identify them), we have no doubt whatever that in their case the proper, the true singular probability, is $1/6$ rather than $1/4$. Or in other words, although they belong to both sequences, we have no doubt that their singular probability is to be estimated as being equal to the frequency of the sequence c rather than b—simply because they are throws with a different (a correct) die, and because we estimate or conjecture that, in a sequence of throws with a correct die, the six will come up in $1/6$ of the cases.

(4) All this means that the frequency theorist is forced to introduce a modification of his theory—apparently a very slight one. He will now say that an admissible sequence of events (a reference sequence, a ' collective ') must always be a sequence of repeated experiments. Or more generally, he will say that admissible sequences must be either virtual or actual sequences which are *characterised by a set of generating conditions*—by a set of conditions whose repeated realisation produces the elements of the sequence.

If this modification is introduced, then our problem is at once solved. For the sequence *b* will not be any longer an admissible reference sequence. Its main part, which consists only of throws with the loaded die, will make an admissible sequence, and no question arises with respect to it. The other part, *bc*, consists of throws with a regular die, and belongs to a virtual sequence *c*—also an admissible one —of such throws. There is again no problem here. It is clear that, once the modification has been adopted, the frequency interpretation is no longer in any difficulty.

Moreover, it seems that what I have here described as a ' modification ' only states explicitly an assumption which most frequency theorists (myself included) have always taken for granted.

Yet, if we look more closely at this apparently slight modification, then we find that it amounts to a transition from the frequency interpretation to the propensity interpretation.

The frequency interpretation always takes probability as relative to a sequence which is assumed as given; and it works on the assumption that a probability is *a property of some given sequence*. But with our modification, the sequence in its turn is defined by its set of *generating conditions*; and in such a way that probability may now be said to be *a property of the generating conditions.*

But this makes a very great difference, especially to the probability of a singular event (or an ' occurrence '). For now we can say that the singular event *a* possesses a probability $p(a, b)$ owing to the fact that it is an event produced, or selected, in accordance with the generating conditions *b*, rather than owing to the fact that it is a member of a sequence *b*. In this way, a singular event may have a probability even though it may occur only once; for its probability is a property of its generating conditions.

Admittedly, the frequency theorist can still say that the probability, even though it is a property of the generating conditions, is equal to the relative frequency within a virtual or actual sequence generated by these conditions. But if we think this out more fully it becomes quite clear that our frequency theorist has, inadvertently, turned into a propensity theorist. For if the probability is a property of the generating conditions—of the experimental arrangement—and if it is therefore considered as depending upon these conditions, then the answer given by the frequency theorist implies that the virtual frequency must also depend upon these conditions. But this means that we have to visualise the conditions as endowed with a tendency, or disposition, or propensity, to produce sequences whose frequencies are equal to the probabilities; which is precisely what the propensity interpretation asserts.

4

It might be thought that we can avoid the last step—the attribution of propensities to the generating conditions—by speaking of mere possibilities rather than of propensities. In this way one may hope to avoid what seems to be the most objectionable aspect of the propensity interpretation: its intuitive similarity to 'vital forces' and similar anthropomorphisms which have been found to be barren pseudo-explanations.

The interpretation of probabilities in terms of possibilities is of course very old. We may, for the sake of the argument, suppress the well known objections (exemplified by the case of the loaded die) against the classical definition of probability, in terms of *equal* possibilities, as the number of the favourable possibilities divided by the number of all the possibilities; and we may confine ourselves to cases such as symmetrical dies or pennies, in order to see how this definition compares with the propensity interpretation.

The two interpretations have a great deal in common. Both refer primarily to singular events, and to the possibilities inherent in the conditions under which these events take place. And both consider these conditions as reproducible in principle, so that they may give rise to a sequence of events. The difference, it seems, lies merely in this: that the one interpretation introduces those objectionable metaphysical propensities, while the other simply refers to the physical symmetries of the conditions—to the equal possibilities which are left open by the conditions.

Yet this agreement is only apparent. It is not difficult to see that mere possibilities are inadequate for our purpose—or that of the physicist, or the gambler—and that even the classical definition assumes, implicitly, that equal *dispositions, or tendencies, or propensities to realise the possibilities in question*, must be attached to the equal possibilities.

This can be easily shown if we first consider equi-possibilities very close to zero. An example of an equi-possibility very close to zero would be the probability of any definite sequence of 0's (heads) and 1's (tails) of the length n: there are 2^n such sequences, so that in the case of equi-possibility, each possibility has the value $1/2^n$ which for a large n is very close to zero. The complementary possibility is, of course, just as close to one. Now these possibilities close to zero are generally interpreted as ' almost impossible ', or as ' almost never realising themselves ', while, of course, the complementary possibilities, which are close to one, are interpreted as ' almost necessary ', or as ' almost always realising themselves '.

But if it is admitted that possibilities close to zero and close to one are to be interpreted as predictions—' almost never to happen ' and ' almost always to happen '—then it can easily be shown that the two possibilities of getting heads or tails, assumed to be exhaustive, exclusive and equal, are also to be interpreted as predictions. They correspond to the predictions ' almost certain to realise themselves, in the long run, in about *half* of the cases '. For we can show, with the help of Bernoulli's theorem (and the above example of sequences of the length n) that this interpretation of possibilities $1/2$ is *logically equivalent* to the interpretation, just given, of possibilities close to zero or to one.

To put the same point somewhat differently, mere possibilities could never give rise to any prediction. It is possible, for example, that an earthquake will destroy tomorrow *all* the houses between the 13th parallels north and south (and *no* other houses). Nobody can calculate this possibility, but most people would estimate it as exceedingly small; and while the sheer possibility as such does not give rise to any prediction, the estimate that it is exceedingly small may be made the basis of the prediction that the event described will not take place (' in all probability ').

Thus the estimate of the *measure* of a possibility—that is, the estimate of the probability attached to it—has always a predictive function, while we should hardly predict an event upon being told no more than that this event is possible. In other words, we do not assume that a possibility as such has any tendency to realise itself; but we do interpret probability measures, or ' weights ' attributed to the possibility, as measuring its disposition, or tendency, or propensity to realise itself; and in physics (or in betting) we are interested in such measures, or 'weights' of possibilities, as might permit us to make predictions. We therefore cannot get round the fact that we treat measures of possibilities as dispositions or tendencies or propensities. My reason for choosing the label '*propensity interpretation*' is that I wish to emphasise this point which, as the history of probability theory shows, may easily be missed.

This is why I am not intimidated by the allegation that propensity is an anthropomorphic conception, or that it is similar to the conception of a vital force. (This conception has indeed been barren so far, and it seems to be objectionable. But the disposition, or tendency, or propensity, of most organisms to struggle for survival is not a barren conception, but a very useful one; and the barrenness of the idea of a vital force seems to be due to the fact that it promises to add, but fails to add, something important to the assertion that most organisms show a propensity to struggle for survival.)

To sum up, the propensity interpretation may be presented as retaining the view that probabilities are conjectured or estimated statistical frequencies in long (actual or virtual) sequences. Yet by drawing attention to the fact that these sequences are defined by the manner in which their elements are generated—that is, by the experimental conditions—we can show that we are bound to attribute our conjectured probabilities to these experimental conditions: we are bound to admit that they depend on these conditions, and that they may change with them. This modification of the frequency interpretation leads almost inevitably to the conjecture that probabilities are dispositional properties of these conditions—that is to say, propensities. This allows us to interpret the probability of a *singular* event as a property of the singular event itself, to be measured by a conjectured *potential or virtual* statistical frequency rather than by an *actual* one.

Like all dispositional properties, propensities exhibit a certain similarity to Aristotelian potentialities. But there is an important difference: they cannot, as Aristotle thought, be inherent in the individual *things*. They are not properties inherent in the die, or in the penny, but in something a little more abstract, even though physically real: they are relational properties of the experimental arrangement—of the conditions we intend to keep constant during repetition. Here again they resemble forces, or fields of forces: a Newtonian force is not a property of a thing but a relational property of at least two things; and the actual resulting forces in a physical system are always a property of the whole physical system. Force, like propensity, is a relational concept.

These results support, and are supported by, my remarks about the role of b—the second argument—in '$p(a, b)$'; and they show that, although we may interpret 'b' as the name of a (potential or virtual) sequence of events, we must not admit every possible sequence: only sequences which may be described as repetitions of an experiment are admitted, and which may be defined by the method of their generation, that is to say, by a generating set of experimental conditions.

5

There is a possibility of misinterpreting my arguments, and especially those of the preceding two sections. For they might perhaps be taken as illustrating the method of *meaning analysis*: what I have done, or tried to do, it could be said, is to show that the word 'probability' is used, in certain contexts, to denote propensities. I have perhaps even encouraged this misinterpretation (especially in section 3) by suggesting that the frequency theory is, partly, the result of a mistaken meaning analysis, or of an incomplete meaning analysis. Yet I do not suggest putting another meaning analysis in its place. This will be clearly seen as soon as it is understood that what I propose is *a new physical hypothesis* (or perhaps a metaphysical hypothesis) analogous to the hypothesis of Newtonian forces. It is the hypothesis that every experimental arrangement (and therefore every state of a system) generates physical propensities which can be tested by frequencies. This hypothesis is testable, and it is corroborated by certain quantum experiments. The two-slit experiment, for example, may be said to be something like a crucial experiment between the purely statistical and the propensity interpretation of probability, and to decide the issue against the purely statistical interpretation.

Note added in proof. In the February number of this *Journal* (1959, **9**, p. 307), Dr I. J. Good has referred to my propensity interpretation. Since this reference contains a misunderstanding, it may be useful, in the interest of clarity, to explain this misunderstanding here.

Dr Good assumes, as basic, a logical or subjective interpretation of $p(a, b)$; we may indicate this by writing $P(a, b)$ and read

$$P(a, b) = r$$

approximately as follows: 'The information b makes it rational to believe in a with a degree of belief equal to r.' Now Good asserts that propensities, in my sense (or, as he prefers to say, physical probabilities), may be defined as special cases of logical or subjective probabilities, as follows. Let H represent all true laws of nature; then we can call

$$P(a, bH) \tag{PP}$$

the physical probability of a given b.

As against this assertion we should realise that many, or perhaps all, of the laws involved in H will be laws asserting a probability; that is to say, H in its turn will be of the form (or it will entail statements of the form)

$$p(a, b) = r, \tag{H}$$

In this case, H is the assertion that under the conditions b, there is a propensity equal to r for a to realise itself, according to my view of the matter.

Now we may accept, as a *principle of logic*, that whenever H is (or entails) '$p(a, b) = r$', then

$$P(a, bH) = r \tag{PP}$$

is *logically true*. Perhaps this is what Dr Good has in mind. But if we accept this principle, there is still a need to interpret the probability statement H. This need is quite independent of (PP), and cannot be replaced by accepting (PP), since H, which occurs in (PP), has to be given some meaning or interpretation.

Dr Good suggests to take H in (PP) as 'understood', and omit it, writing

$$P(a, b) = r, \tag{P}$$

provided we have agreed that this should mean exactly the same as (PP).

Now (P) looks, of course, very much like H; and this may explain why Dr Good takes it for H (that is, for my propensity statement). Yet (P) is very different from H. This may be best seen as follows.

According to our *principle of logic* (PP) or (P) will be *logically true whenever* $H =$ '$p(a, b) = r$'; therefore, logical probability of (P) will be equal to 1. But nobody will assert that the logical probability of the empirical statement H equals 1. (On the contrary, if H is the product of all laws of nature—which we may never discover—its logical probability will be, according to all authors, very small; and according to some authors—for example myself—it will be zero.)

Thus $H \neq$ (P); and the identification of the logical statement (P) with the empirical statement *H* about propensities is mistaken: it is impossible to subsume in this way propensities (or any other objective probabilities) under logical or subjective probabilities.

Appendix

In conclusion I wish to add a historical remark, and some remarks on axiom systems for probability.

The distinction between subjective, logical, and objective (statistical) interpretations of probability which I made in 1934 in my *Logic of Scientific Discovery* (referred to in what follows as '*op. cit.*'), section 48, was used for arguing the thesis that within physics, at any rate, only statistical probability is relevant. (I now wish to replace, in this thesis, the statistical by the propensity interpretation.) But I made considerable use, in this work, of the logical interpretation also (especially in order to show that content = logical improbability). In 1938 I argued in favour of a '*formal*' theory of probability, based upon an axiom system 'constructed in such a way that it can be . . . interpreted by any of the proposed interpretations . . . and by some others also (*op. cit.* p. 320). In looking for these other interpretations, with a view to the needs of quantum theory, I found the propensity interpretation. I also found that formerly (in *op. cit.* section 71, especially p. 212) I had explicitly argued against accepting an interpretation of this kind.

In my own mind, the freedom of operating with different interpretations was closely connected with the adoption of a formal or axiomatic treatment of probability, as envisaged, for example, by Kolmogorov (see *op. cit.*, p. 327).

In Kolmogorov's approach it is assumed that the objects *a* and *b* in $p(a, b)$ are sets (or aggregates). But this assumption is not shared by all interpretations: some interpret *a* and *b* as states of affairs, or as properties, or as events, or as statements, or as sentences. In view of this fact, I felt that in a formal development, no assumption concerning the nature of the 'objects' or 'elements' *a* and *b* should be made; and it appeared to me desirable not even to assume that these 'objects' or 'elements' satisfy the laws of Boolean algebra (although I found that they do): it became desirable to assume axioms of a 'metrical' character only. Another point was that it became desirable to construct a theory in which the formula (4) mentioned in the second footnote to the present paper, i.e.

$$p(a, c\bar{c}) = 1$$

was a theorem: this, it turned out, was a condition of adequacy for the logical interpretation, and desirable on general grounds.

The first system of the kind here described was published by me in this *Journal*, 1955, **6**, 56, and I simplified its axioms in 1956.[1]

This simplified system, and a number of variants, were discussed in some detail in appendix * iv of *op. cit.* I shall state here one more of its variants.[2] The system uses as undefined terms the class S of the ' objects ' or ' elements ' a, b, . . .; the product element ab of the elements a and b; and the complement-element $\bar{a}$ of the element a. There are three axioms.[3]

Postulate A. If a and b are elements of S then $p(a, b)$ is a real number, and the following axiom holds:

A $$(\mathrm{E}c)(\mathrm{E}d)\, p(a, b) \neq p(c, d).$$

Postulate B. If a and b are in S, then ab is in S, and the following axiom holds:

B $$(p(a, a) = p(bc, d) \;\&\; p(bc, c) = p(d, c)) \rightarrow p(ab, c) = p(a, d)p(b, c) \leq$$
$$\leq p(a, c).$$

Postulate C. If a is in S, then $\bar{a}$ is in S; and provided b, c, and d are also in S, the following axiom holds:

C $$p(a, a) \neq p(b, c) \rightarrow p(a, c) + p(\bar{a}, c) = p(d, d).$$

Both B and C are immediate consequences (using only substitution and *modus ponens*) of the following more complicated formulae BD and CD which, however, have the advantage that they may be regarded as *explicit definitions* of ab and of $\bar{a}$, respectively (BD is an improved version of the corresponding formula in *op. cit.* p. 336):

BD $$p(ab, d) = p(c, d) \longleftrightarrow (e)(\mathrm{E}f)(p(a, d) \geq p(c, d) \leq p(b, d) \;\&\; (p(a, d) \geq$$
$$\geq p(a, a) \leq p(b, d) \rightarrow p(a, a) \leq p(c, d)) \;\&\; ((p(b, e) \geq p(a, a) \leq$$
$$\leq p(d, e) \;\&\; (p(b, f) \geq p(a, a) \leq p(d, f) \rightarrow p(a, a) \leq p(e, f))) \rightarrow$$
$$\rightarrow p(a, e)\, p(b, d) = p(c, d))).$$

CD $$p(\bar{a}, c) = p(b, c) \longleftrightarrow (d)(e)(p(a, a) \neq p(d, c) \rightarrow p(a, c) + p(b, c) =$$
$$= p(e, e)).$$

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[1] See my paper ' Philosophy of Science: A Personal Report ', in *British Philosophy in the Mid-Century*, edited by C. A. Mace, 1956; the axiom system can be found on p. 191.

[2] As compared with the system of *op. cit.*, p. 332, the present system combines, in B, A2 with B1 and B2. C is the C^3 of p. 334.

[3] The following *abbreviations* are used: ' (x) ' for ' for all elements x in S '; ' $(\mathrm{E}x)$ ' for ' there is at least one element x in S such that '; ' . . . → . . . ' for ' if . . . then . . . '; ' ⟷ ' for ' if and only if '; ' & ' for ' and '.

Aesthetically both of these definitions suffer from the disadvantage that one half of the double arrow is redundant: in deriving the axioms B and C we have to use the arrow from the left to the right only. Definition Cd, which can replace CD, is free from this disadvantage.[1]

Cd $\quad p(\bar{a}, b) = p(c, c) - p(a, b) \longleftrightarrow (\mathrm{E}d)p(c, c) \neq p(d, b).$

In BD we may put '$p(e, e)$' for the second occurrence of '$p(a, a)$'. (This makes A3, *op. cit.* p. 332, deducible from BD.) We may then simplify CD or Cd, writing '$p(a, a)$' for '$p(e, e)$' or '$p(c, c)$'.

Compared with the system in *op. cit.* p. 332, both B and BD incorporate A2. Incorporating A2 with *any* of the axioms has the advantage that the resulting system is 'completely metrical' in the sense that *the independence of all axioms can be proved with the help of examples that satisfy Boolean algebra.* (Thus 'completely metrical' is a stronger property of a system than 'autonomously independent' in the sense of *op. cit.* pp. 343-344.) We can achieve a completely metrical system without sacrificing the 'organicity' (in the sense of the Warsaw School) of our axioms, by retaining all the axioms (including B1) of *op. cit.* p. 332 except A2; for A2 can be incorporated organically with B2, for example, by omitting '$\leqslant p(a, c)$' from the formula B, above. Alternatively, we can leave even B2 in its original form and incorporate A2 organically with postulate AP of p. 333, as follows:

AP $\quad p(a) = p(a, b) - p(a, c) + p(a, d),$

provided $p(b, c) = p(c, b) = p(d, e)$ for every e in S.

In this case AP—that is to say, a definition of absolute probability—becomes an integral and indispensible part of the system.

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[1] This is due to the fact that Cd is logically stronger than C since it allows us to replace A by a logically weaker conditional formula; for in the presence of Cd, we may add to A the proviso, 'provided $(\mathrm{E}e)(\mathrm{E}f)\, p(e, f) \neq 0$' (or in words, 'provided not all probabilities are equal to zero'). The strength of Cd is due to the fact that, with the arrow from right to left only, Cd would be the same as C, while the arrow from left to right allows us, in addition, to deduce that not all probabilities are zero.

It may also be mentioned here that the condition of B, as formulated in the text, may be replaced by the (stronger) condition, '$(e)\, p(bc, e) = p(d, e)$'. (This replacement corresponds to the transition from formula $A2^+$, on p. 335 of *op. cit.*, to A2 on p. 332.)

DISCUSSIONS

Notes on P. K. Feyerabend's Criticism of Positivism

In a recent paper[1] Dr P. K. Feyerabend puts forward some new criticisms of what I shall call 'old-fashioned positivism' in an attempt to make plausible a realistic interpretation of theoretical quantum mechanics. There are, however, various muddles and lacunae in his argument. Feyerabend's paper abounds in technical terms and so is exceedingly difficult to follow. My main purpose is to show that when the technical terms are removed and the arguments rephrased in ordinary English they lose a great deal of their apparent force.

So far as I can understand him Feyerabend seems to be arguing that the way we tell whether a statement is true is independent of, or partially independent of, the meanings of the words in that statement. He seems to be arguing against the verification theory of meaning which went with an old-fashioned positivist's denial that the meanings of scientific terms owed anything to theory. I think that the point of Feyerabend's raising of this old issue is that he believes that if he can establish the independence of method of verification and the meaning of a statement then he will have shown that features of a theory are, or are candidates for the status of, features of the world independent of the actual facts needed to verify descriptive statements in which they appear. In particular I think he wants to show that the special features of quantum mechanical theory are, or are candidates for the status of, features of the world. If this is his argument it is simply fallacious. It does not follow from the independence of testing procedures and the meaning of statements tested that theoretical concepts *have* this candidate-for-reality status, just that they *could* have it. I believe myself that *some* features of *some* theories do have this status but this cannot be shown by Feyerabend's kind of general argument, it must be shown in detail.

I shall argue against Feyerabend:

(*a*) that only some descriptive statements involve terms whose meanings depend partly on theory.

(*b*) that there are some features of theory which do not have candidate-for-reality status. I shall try to show that the uncertainty relation between complementary physical quantities could be a feature of this sort.

In giving a description of something we use words in two different ways. Let us suppose we are asked to describe what happens when two suspended cork balls are given a like charge.

D (1). Two cork balls which have both been touched by a piece of fur-rubbed ebonite *move apart*.

The expression 'move apart' which occurs in this description could be taught to someone not familiar with it by using this happening as a paradigm case example. Hence to use the expression 'move apart' correctly here involves nothing over and above the happening to be described. The same could be said of any of the expressions used in this description.

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[1] P. K. Feyerabend, 'An Attempt at a Realistic Interpretation of Experience', *Proceedings of the Aristotelian Society*, 1957-58, 58, 143 ff.

D (2). Two cork balls which have both been touched by a piece of fur-rubbed ebonite *repel each other*.

The expression ' repel each other ' could not be adequately taught in this situation, for it is paradigmatically correct in another situation. It could be taught, for example, at an historical film where the defenders *repel* the besiegers from the ramparts of their town. Its use in D (2) brings with it the notion of a *force* of repulsion wholly absent in the use of ' move apart '. It is both descriptive and explanatory.

Both descriptions serve adequately to describe the happening but that which could be taught by a paradigm case in the situation to be described can introduce nothing of the theory which explains the moving apart of the cork balls. The theory has to be introduced by the use of a metaphor like ' repel '. Provided that you use only expressions which could be taught in the situation described you have given a description *wholly* independent of theory.

How does this apply to Feyerabend's argument? If he does not distinguish between scientific descriptions that are of the type D (2) and those that are of the type D (1) then he could argue that we could alter our theory of electrostatics and correspondingly substitute some other word, say ' hates ' for ' repels ' and still find out the truth of the statement in just the same way. If D (1), D (2), and D (3) (this last being ' Two cork balls which have been touched by a piece of fur-rubbed ebonite *hate each other* ') were synonymous this might well be true. However, they are not synonymous, for it is not just a matter of theoretical convenience that we replace ' move apart ' by ' repel each other ' and not by ' hate each other '. That the cork balls are not *brought to life* by the touch of the ebonite wand is a matter of fact; a fact which is reflected in our theory and which finds expression in our use of ' repel ' rather than ' hate '. The single experiment with cork balls will serve to verify D (1) but it will not serve to distinguish D (2) from D (3). Method of verification may not determine the meanings of words but the meanings of words certainly influences the method of verification when the use of any of these words in a description owes anything to theory. The way we tell whether a statement is true is not independent of the meanings of the words used in it. If when Feyerabend says ' we shall explain how it is possible for the interpretation of a language to change without any perceptible effect upon its characteristic ' he means that the way we set about telling if a statement is true is wholly independent of changes in the meanings of the words used in it then he is conflating descriptions of the type of D (1) with descriptions of the type of D (2) and D (3). It follows then that this part of his argument is quite unsound.

However his main thesis could be maintained independently of these fallacious linguistic arguments. I think that he is aiming at showing that the strange features of quantum mechanical theory are not to be regarded as convenient mathematical devices but as real features of the world, or as reflecting real features of the world. I am not sure which of these claims he wishes to make. I shall therefore take him to be making the weaker claim that the unusual features of quantum mechanics reflect real features of the world. The stronger claim hardly deserves consideration.

When the bacteriological basis of disease was discovered a host of ailments were found to have determinable microbiological causes. Those diseases which resisted the efforts of investigators were still thought to have the same kind of causes. The old word ' virus ' was appropriated to name a class of microbiological entities which were postulated by theory but which were too small to be identifiable under a microscope. As everyone knows the invention of the electron microscope transformed the status of viruses from theoretical postulates to real things. The virus was once part of a microbiological model for understanding and treating diseases, but this model has now been shown to be the real fine structure of nature at this point. The virus was once a candidate for reality and has now been shown to be real. It was a candidate for reality just because it was of a type with things which had been shown to be real.

Is quantum mechanics like microbiology? Can we expect that the invention of new instruments will lead us to say that electrons have changed their status, as the invention of the electron microscope led us to say that viruses had changed their status? There is this difference to begin with: viruses are things of a type with which we are already familiar, minute living organisms; but electrons are apparently not of a type with anything with which we are familiar. Viruses were *modelled on* microbes but electrons have had to be modelled on *two* quite different types of things with which we are familiar, particles and waves. Must we say that we have discovered a *new type* of thing, or are electrons just a new type of model?

Sometimes the choice of a certain explanatory model forces us to give our theoretical entities queer (queer with respect to the everyday world) properties. People who persisted in trying to understand combustion by the theory that a substance was *given off* in burning found themselves committed to giving their theoretical entity negative weight. It might well have been argued that though the phlogiston theory explained certain facts about combustion the theoretical substance involved had to be given such bizarre properties that it couldn't be a candidate for reality. It may be that the ' queer ' features of quantum mechanics derive from a persistence with an unsuitable model.

The issue is complicated in quantum mechanics by the use of an advanced mathematical form of description for sub-atomic ' particles ' for descriptions in mathematical form are peculiarly liable to be misunderstood. Einstein noticed that he was most successful in finding the algebraic formulae that he needed when he chose the *simplest* of any alternative forms. He went on to suppose, as he tells us in *The Meeting of East and West*, that this success was due to the fact that nature always tended towards the simplest forms. But of course nature can neither be said to be simple nor complex, it is what it is. Only the *form of description* supplied by the scientist can be simple or not simple. Einstein mistook a feature of the mathematical description for a feature of the world. Perhaps the apparently *peculiar* properties of sub-atomic entities are features of the quantum mechanical formalism in which they are described. There seem to me then to be three alternative ways of looking at any problem of this kind, alternatives which Feyerabend does not seem clearly to distinguish.

(i) The entities of quantum mechanics are candidates for reality but because they do not have the properties of the things with which we are familiar we say we have discovered or are on the road to discovering a new *type* of thing altogether.

(ii) The quantum mechanics is a model which describes and explains various facts about spectra, radiation, and so on but it postulates theoretical entities that have such peculiar properties that they couldn't be candidates for reality.

(iii) The quantum mechanics is characterised by a form of description, with certain peculiar *mathematical* features, and though electrons and other sub-atomic entities are candidates for reality we should not expect them, when more closely investigated, to actually have the peculiar features which we needed to introduce to produce a simple, concise, and consistent formal system. One is reminded in this connection of those people who argued that time must be unreal because Minkowski found it convenient to multiply time-variables by *i*.

I do not see how a primarily linguistic argument such as Feyerabend gives can settle which of these possibilities we are to opt for. The features which are puzzling about quantum mechanics are primarily features of the relations between various properties which we *assign* to the theoretical entities. Feyerabend says (footnote 20, p. 161) 'the magnitudes (properties) of classical physics can be determined at any time with any required precision. On the other hand quantum mechanical entities are complementary in the sense that at a given time they are able to possess only some of their possible properties.' First two matters of detail: (*a*) it is the *properties* of quantum mechanical entities that are complementary, not the entities themselves; (*b*) it is not a question in the first instance of whether these entities *possess* some properties and lack others, but of whether we can *measure* two complementary properties simultaneously. If we design our theoretical entities so that they do only possess some but not all of their possible properties simultaneously this is a choice on our part. We do not *have* to say this.

My own feeling is that the principle of uncertainty that forbids precise measurement of complementary properties simultaneously is a principle of something the same sort as Einstein's principle of simplicity. That is to say it is a feature of the formalism and a consequence of the model we choose rather than a candidate for reality. It arises when we explain certain facts about spectra and radiation by using a particle-model. This model will only explain these facts when the mathematical description of the properties of the postulated particles shows them to have this curious complementarity. But we don't *have* to use a particle-model. It just happens to be traditional and sometimes convenient. This could only be settled by a detailed examination of the way complementarity enters into the theory and what relation it bears to the facts to be explained.

It seems unlikely that anything much is to be gained at present by arguing about which of the three possibilities above we should choose to accept. What is important is to see that there are various possibilities. I am inclined to think that (i) and (iii) are more reasonable than (ii), that is I do not think that it would be advisable in the light of the history of science to make flat statements about the impossibility of finding things equally as strange as the theoretical entities of quantum mechanics. But their strangeness should not be accepted too lightly. Nothing in Feyerabend's paper seems to me to settle one way or another which of the remaining alternatives is true. So far as I can see the sort of arguments he advances and the sort of terms he couches them in, by obscuring the distinctions between model, metaphor, simile and fact, do not advance our understanding of either the philosophical or the scientific issues involved.

In addition to the substantial points I have already discussed there are some other difficulties which appear particularly in Feyerabend's choice of technical terms. One of these is a minor matter but two are more serious. The minor, though confusing, matter is this: in his argument to what he calls the 'stability-thesis', which he claims is a characteristic doctrine of old-fashioned positivism, Feyerabend argues from the statement that an interpretation does not depend upon the 'state of the "theoretical" superstructure'[1] to the stability-thesis 'interpretations do not depend upon the status of our theoretical knowledge'.[2] It may be that the change from 'state' to 'status' is a slip but I am inclined to think that an old-fashioned positivist, far from feeling that his doctrine had been summed up by either of these statements, would have been disposed to disagree with both of them. He would have disagreed with the former on the grounds that the state of one's theory is a historian's and not a logician's matter, and with the latter on the grounds that interpretations *do* depend on the status of our theoretical knowledge. I mean, suppose we treat our theoretical knowledge as an inference-device, then this decision as to its status alters our views on interpreting it. Thus Feyerabend does not make it quite clear what he takes the characteristic doctrine of positivism to be.

The second, more serious muddle, occurs in Feyerabend's use of the word 'ontological'. He uses the phrase 'ontological consequences of *L*', where *L* is some 'language' to describe the assumptions which, for example, underlie the use of numbers for counting. He gives as examples that objects are discreet and that the result of counting is independent of the order of counting. These he claims are 'ontological consequences of *L*' where *L* is presumably the list of natural numbers. But these assumptions are neither ontological (concerned with claims about what objects there *are*) nor consequences of *L*, for the list of natural numbers does not have any consequences. I think what he actually means by this curious expression is 'methodological presuppositions of the use of a certain kind of vocabulary for describing things'. The confusion between 'consequences', 'presuppositions', and 'assumptions' (something different again) goes right through Feyerabend's paper. Indeed I think his argument could be shown to fail on this ground alone.

Connected with this is another terminological confusion which appears in several places in Feyerabend's paper. For instance Feyerabend says at one point 'every positivistic observation language is based upon a metaphysical ontology'.[3] What this seems to mean is that for any given vocabulary used for describing things there is involved in its use a certain set of assumptions about the nature of things which is presupposed whenever we use this vocabulary. But these assumptions, if they are identical with the 'ontological consequences' of page 148, which I think they *are*, are certainly not statements from a 'metaphysical ontology' in any of the senses of 'metaphysical' or 'ontology' that I know. They neither state the *existence* of a certain class of objects (except in the trivial sense that any non-vacuous statement must have something to be about), nor do they, *a fortiori*, state ways of arriving at the existence of any class of objects solely by reasoning. I must insist that statements of the kind which Feyerabend calls 'ontological consequences' and which are supposed by him to form a 'metaphysical ontology' should properly be called either assumptions or presuppositions of a certain way of talking. I am not sure what is the proper expression here but that is a matter for detailed investigation. The mistake of classifying these assumptions as part of a 'metaphysical ontology' is not just a verbal mistake, for the expression 'metaphysical ontology' produces a kind of intellectual shudder which is not produced by the use of words like 'assumption' and 'presupposition'.

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[1] P. K. Feyerabend, 'An Attempt at a Realistic Interpretation of Experience', *Proceedings of the Aristotelian Society*, 1957-58, **58**, 147n

[2] Ibid., 147

[3] Ibid., 149

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The Falsifiability of the Lorentz-Fitzgerald Contraction Hypothesis

In his latest book *The Logic of Scientific Discovery*, K. R. Popper cites the Lorentz-Fitzgerald contraction hypothesis as 'an example of an unsatisfactory auxiliary hypothesis', claiming that it 'had no falsifiable consequences but merely served to restore the agreement between theory and experiment—mainly the findings of Michelson and Morley'.[1]

The charge of being *ad hoc* levelled here by Professor Popper against the Lorentz-Fitzgerald contraction hypothesis is *wholly incorrect* but has persisted in the literature on the philosophy of science for the past half century. I therefore wish to give a refutation of the thesis of non-falsifiability on the basis of a comparison of the reasoning underlying the Kennedy-Thorndike experiment [2] with the design of the Michelson-Morley experiment.

Specifically, I shall show that, contrary to Professor Popper's contention, (i) the coupling of the Lorentz-Fitzgerald contraction hypothesis with the classical aether theory entails a positive outcome of the Kennedy-Thorndike experiment *differing quantitatively* from the positive result required by the aether theory *without* the Lorentz-Fitzgerald auxiliary hypothesis, and (ii) while entailing the null outcome produced by the *Michelson-Morley* experiment, the Lorentz-Fitzgerald version of the aether theory *rules out* the negative result which was actually yielded by the *Kennedy-Thorndike* experiment.

The essential difference between the apparatus used in these two experiments is the following: as measured by rods in the laboratory, the horizontal and vertical arms of the Michelson interferometer used in the *Kennedy-Thorndike* experiment are *not* equal but are made as *different* in length as possible, so as to assure a considerable difference in the travel times of the two partial beams from the source to the point at which they recombine to produce interference fringes. On the other hand, the horizontal and vertical arms of the apparatus used in the *Michelson-Morley* experiment are *each* of the *same* length l as measured by the rods in the laboratory.

Accordingly, in the *latter* experiment the classically-expected round-trip times T_v and T_h for the vertical and horizontal arms are given respectively by

$$T_v = \frac{2l}{\sqrt{(c^2 - v^2)}} \qquad \text{and} \qquad T_h = \frac{2l}{\sqrt{(c^2 - v^2)}} \cdot \frac{1}{\sqrt{(1 - \beta^2)}},$$

where 'c' represents the velocity of light, 'v' the velocity of the apparatus relatively to the aether, and $\beta \equiv v/c$. Now, without a Lorentz-Fitzgerald contraction hypothesis, the initial time difference $T_h - T_v$ between the two partial light beams would be expected to change in the course of the rotation of the apparatus through 90° in the Michelson-Morley experiment. And thus a shift in the interference fringes corresonding to this change was anticipated. But, once the aether theory is amended by he introduction of the Lorentz-Fitzgerald contraction, the length l in the expression or T_h must be replaced by the length $l\sqrt{(1-\beta^2)}$. As a consequence of the introduction of this auxiliary hypothesis, T_h becomes *equal* to T_v, and the difference between he round trip times of the two partial beams is zero throughout the Michelson-Morley experiment, in conformity with its null result.

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[1] K. R. Popper, *The Logic of Scientific Discovery*, London, 1959, p. 83

[2] Cf. R. J. Kennedy and E. M. Thorndike, *Physical Review*, 1932, **42**, 400, and W. Panofsky and M. Phillips, *Classical Electricity and Magnetism*, Cambridge, 1955, p. 236

It will be noted that the equality of the *terrestrially-measured* lengths of the two arms is a *necessary condition* for the vanishing of the difference between the two round trip times in the Lorentz-Fitzgerald account of the Michelson-Morley experiment. But precisely this necessary condition is *not* fulfilled in the Kennedy-Thorndike experiment in which the *terrestrially-measured* lengths of the vertical and horizontal arms have the *unequal* values L and l respectively. Thus, *upon assuming the Lorentz-Fitzgerald contraction*, the difference between the travel times of the two light beams of the Kennedy-Thorndike experiment is given by

$$T_v - T_h = \frac{2L}{\sqrt{(c^2-v^2)}} - \frac{2l}{\sqrt{(c^2-v^2)}} = \frac{2}{\sqrt{(c^2-v^2)}}(L-l).$$

Instead of vanishing throughout the experiment, this time difference *varies* with the diurnally and annually changing velocity v of the apparatus relatively to the fixed aether. Moreover, if we do *not* assume a Lorentz-Fitzgerald contraction, the difference between the two travel times of the Kennedy-Thorndike experiment has the *different* value given by

$$T_v - T_h = \frac{2L}{\sqrt{(c^2-v^2)}} - \frac{2l}{\sqrt{(c^2-v^2)}} \cdot \frac{1}{\sqrt{(1-\beta^2)}} = \frac{2}{\sqrt{(c^2-v^2)}}\left(L - \frac{l}{\sqrt{(1-\beta^2)}}\right)$$

And this difference is likewise a function of the diurnally and annually varying velocity v of the apparatus. It is apparent that *the Lorentz-Fitzgerald contraction hypothesis has a falsifiable bearing on the outcome of the Kennedy-Thorndike experiment* and that *this experiment could have detected any existing velocity of the apparatus relatively to the aether even on the assumption of a Lorentz-Fitzgerald contraction*: if the period of the light source does *not* itself depend upon this velocity, the appropriate annually and diurnally changing time difference given above would have given rise to corresponding observable shifts in the interference fringe pattern. Contrary to both the original and the Lorentz-Fitzgerald version of the aether theory, Kennedy and Thorndike found that such shifts did not materialise.

In addition to the Kennedy-Thorndike experiment, the following experiment suggested by C. Møller and others qualifies as a test of the Lorentz-Fitzgerald contraction within the framework of *pre*-relativistic conceptions: although a *terrestrial* observer could *not* detect a Lorentz-Fitzgerald contraction in a Michelson-Morley experiment, ' an observer at rest in the aether outside the earth would, however, in principle be able to observe the shortening and he would find the earth and all objects on the earth contracted in the direction of motion of the earth '.[1]

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[1] C. Møller, *The Theory of Relativity*, Oxford, 1952, p. 29. For a discussion of the *important differences* between the *pre*-relativistic and the relativistic conceptions of the status of any contraction disclosed by the extra-terrestrial experiment suggested here by Møller, cf. H. Reichenbach, *The Philosophy of Space and Time*, New York, 1958, §31, and A. Grünbaum, ' Logical and Philosophical Foundations of the Special Theory of Relativity ', *American Journal of Physics*, 1955, **23**, 460-464.

Lorentz himself was quite clear that the contraction hypothesis would not suffice to make the body of experimental findings known at the time conform to the expectations of the aether theory. He therefore invoked the *further* auxiliary hypothesis known as the Lorentz-Larmor-Poincaré time dilation, construed as issuing in a *spurious* ' local ' time in all systems moving relatively to the aether.[1] Only a version of the aether theory incorporating *both* the Lorentz-Fitzgerald contraction *and* the Lorentz-Larmor-Poincaré time dilation is vulnerable to the *ad hoc* charge, which tradition and Professor Popper have unjustly levelled against the first of these two auxiliary hypotheses alone.

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TESTABILITY AND ' AD-HOCNESS ' OF THE CONTRACTION HYPOTHESIS

PROFESSOR GRÜNBAUM'S most lucid explanation of the Kennedy-Thorndike experiment (1932) is a very welcome contribution to the discussion of the problem whether or not the introduction of a new hypothesis is to be regarded as a potential advance. In the passage of my book referred to by Professor Grünbaum I said that the goodness of an auxiliary hypothesis would depend on whether it is independently testable, or perhaps merely rescuing a refuted theory. Professor Grünbaum points out, rightly, that I have been guilty of uncritically accepting (or possibly even spreading) the myth that the Lorentz-Fitzgerald contraction hypothesis was not testable. Professor Grünbaum's correction shows that this hypothesis was testable and thus not *ad hoc* to the degree I believed. Accordingly *it was an advance*. But it was, of course, more *ad hoc* than special relativity. In other words, we have here an excellent example of ' degrees of *ad-hocness* ' and of one of the main theses of my book—that *degrees* of *ad-hocness* are related (inversely) to degrees of testability and significance.

K. R. POPPER

COMMUNICABLE AND INCOMMUNICABLE REALITIES

THE purpose of this note is not to resume the worn-out discussion of the incomparability of the ' subjective ' quality of perception and feeling in different individuals. Our point is rather that even if one should assume the identity of sensory and perceptual qualities in organisms equipped with identical sensory and perceptual mechanisms and nervous systems, the incommunicability of certain realities emerges even on the basis of logical considerations.

Gabriel Marcel, in his *L'Homme Problématique*, presents a man who has lost everything in the war and asks himself, ' Who am I ? ', ' What does all this mean ? '.

> ' Qui suis-je ? Quel sens a tout cela ? Est-ce qu'un autre homme, un individu lui aussi, sera en mesure de l'éclairer ? Il pourra faire tous ses efforts pour introduire notre homme dans sa propre vie, dans son univers à lui, il pourra même

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[1] Cf. Møller, op. cit. p. 30

> partager avec lui ce qu'il possède. Seulement cette vie, cet univers, ne sont pas interchangeables, et cela même ne serait pas une réponse. Chose étrange, c'est justement parce que cet autre homme sait qui il est et pourquoi il vit qu'il ne peut pas répondre à la question de l'homme de la baraque. Cette question, il ne peut la comprendre qu'imparfaitement, car il ne sait pas ce que c'est que d'avoir perdu sa patrie, sa maison, avec tous ceux qu'on aime. Et même s'il avait perdu tout cela, il se trouverait dans la même situation, plus proche, par conséquent, mais il est vraisemblable qu'alors il poserait la même question, loin de pouvoir y répondre.'[1]

'Who am I ? What does all this mean ? Will another person, himself also an individual, be in a position to answer the question for the fellow in the barracks ? He may try his best to orient our poor war victim in his proper life, in his own universe, and may even share with him whatever he has. But the trouble is that this life and this universe are not interchangeable between persons. And even to say so would not be a satisfactory answer to the unfortunate fellow.

' How remarkable it is ! It is because this other man knows who he is and for what purpose he lives—that he is not able to answer the question of the fellow in the barracks. He understands the question only imperfectly, because he does not know what it is to have lost one's own homeland and one's own house with all that one loves. And if he himself had lost all these, he would find himself in the same situation as the man in the barracks. He would then be closer to the misfortuned. But it is probable that he would then ask the same question far from being able to answer it.'

Suppose that Mr Able advocates that everything in the universe of our thoughts and experiences can be communicated between persons, given enough time and enough inventions of communication methods, and that the only limitations to the communicability in his universe are of a practical nature such as channel capacity, inadequacy of the coding systems, insufficiency of our inventiveness to improve such shortcomings, and the impossibility of making observations without interfering with the system under observation. On the other hand Mr Zeeland maintains that in his universe there are things which cannot be communicated between all persons, but which may be ' resonated ' between those who have had or are having the same experiences. As examples he gives such realities as the spirit of the Hungarian revolution, the terror of the concentration camps, suicidal despair, aspiration for freedom, a certain sense of humour, a certain degree of seriousness, and even draws your attention to a more theoretical side of the matter by quoting Edmund Husserl:

> ' Ein Taubgeborener weiss, dass es Töne gibt, dass Töne Harmonien begründen und dass in diesen eine herliche Kunst gründe ; aber verstehen, *wie* Töne das anstellen, *wie* Tonkunstwerke möglich sind, kann er nicht. . . . Es wäre absurd, auf grund seines Wissens das Wie der Tonkunst zu deduzieren. . . . Und es gilt offenbar nicht bloss für das Problem der transzendenten Objektivität, sondern für Aufklärung jeder Möglichkeit.' (*Die Idee der Phänomenologie*).[2]

' A congenital deaf-mute knows that there exist tones, that tones build harmonies, and that a marvellous art originates from them. But he cannot understand *how* it sounds, nor *how* the works of music are possible. . . . It would be absurd to infer on the basis of his knowledge the *how* of the musical art. . . . And this applies not only to the problem of transcendental (intersubjective in Husserl's terminology) objectivity, but also to the elucidation of every possibility.'

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[1] *L'Homme problématique*, Aubier, Paris, 1955, p. 13

[2] Edmund Husserl, *Die Idee der Phänomenologie*, Nijhoff, The Hague, 1950, pp. 38–39

The deduction of the phenomenologically immanent from the theoretical knowledge of the transcendent is impossible. Mr Able then replies that in his own universe all these things are amenable to scientific measurement and to analogical extrapolation of his own emotional reality. Mr Zeeland protests that that would be reducing the whole universe to something like its geometrical projection on to a subuniverse of fewer dimensions (i.e. something like reducing three-dimensional movements to their shadows on a two-dimensional plane). Is there any way to settle this controversy ? Let us first examine the matter from the logical point of view.

Mr Able's universe is the universe of communicable entities. That all the reality of his universe is communicable, seems to be his postulate. He may be a deductionist or an inductionist. Therefore we are not justified in assuming that he is necessarily deducing his philosophy from this particular postulate or from any other particular postulates. But we can at least ask whether his philosophy is consistent. What he does in his scientific activities is to start with communicable entities and manipulate and combine them using operations which are in his universe, i.e. those operations as well as their results are definable with communicable terms. The products of his activities are then again something communicable. Thus in his system Mr Able *never encounters* anything incommunicable, nor does he *need* anything incommunicable. He gets in no contradiction by rejecting everything incommunicable. He operates in a closed system, in the same sense that the set of positive integers is closed with respect to addition and multiplication, the set of positive and negative integers together with zero is closed with respect to addition, subtraction, and multiplication, the set of rational numbers is closed with respect to addition, subtraction, multiplication, and division, and the set of complex numbers is closed with respect to algebraic operations.

To advance from the set of positive integers to the set of positive and negative integers, one needs to introduce, either directly negative integers, or a new operation: subtraction. In order to move from a closed set to a more comprehensive closed set, one has to introduce either new element(s) to operate on, or new operation(s) which will yield new element(s) from old element(s). By rejecting the introduction of new things from outside, one never gets out and never needs to get out from the closed system. If Mr Able has never experienced in his life anything which was not amenable to interpersonal communication, he can deny the existence of incommunicable realities. Even if he had the good will to accept the existence of incommunicable realities because someone else says that they exist, he could never perceive them with his communication means (which are the only guarantee for him for ascertaining the existence of anything), because incommunicable realities are by definition incommunicable. Mr Able may take it philosophically, and give the 'incommunicable reality' the place of 'Ding-an-sich'. But this Ding-an-sich is something entirely useless to him and is what he is denying in the first place. Therefore, Mr Zeeland, while understanding Mr Able's situation, is unable to communicate to him what he means by incommunicable realities.

Mr Zeeland is right in comparing Mr Able's situation with geometrical projection. The projected image may or may not give a consistent interpretation of the phenomenon. If the interpretation is inconsistent, it is possible to invent rationalisations within the reduced dimensionality rather than going back to the original unreduced dimensionality. For example, let us consider a physical system in a three-dimensional orthogonal co-ordinate system without force field. Let us suppose that two particles of an equal mass fly, collide, and bounce apart again. We assume that the kinetic energy is conserved. If this situation is projected perpendicularly to the X-Y plane, the shadow-universe shows that the kinetic energy is conserved if and only if the sum of the squares of the Z-components of the velocity of the two particles remains the same before and after the collision. Otherwise the shadow phenomenon does not obey the law of the conservation of kinetic energy. But it is possible to invent rationalisations such as potential energy, energy of the internal state of the particles, conversion of energy to and from matter, etc., such that the sum of all kinds of energy and matter is constant.

In order that our discussion does not degenerate to mental acrobatics of empty abstraction, let us examine some concrete cases.

Mr Zeeland asks Mr Able : 'How would you feel if you were a fish ?'

Mr Able : 'I can *analyse* the electro-encephalogram and oscillogram of the brain and the neurons of the fish, and study all its response patterns to various stimuli, and use anthropological and psychoanalytic intuition. Then I can find out how a fish feels.'

Mr Zeeland : 'By the same reasoning, if you are born deaf you can still *look at* the music through an oscilloscope and a harmonics analyser, study the theory of harmony, counterpoint, and orchestration, analyse the notes, examine the music instruments, and even compose music according to music theory, look at the results in an oscilloscope, receive comments from the audience, etc. But the trouble is that you never hear the music yourself. You have to have normal auditory organs and an auditory centre in the brain in order to hear the music. Similarly you have to have the fish brain, the fish neurons, and the fish body in order to know how a fish feels. But when you have these you have no longer your human brain to *analyse* your fishy feelings ?'

Mr Able : 'All right ; but that's about fish. We started our discussion with human communication. Let us go back to our main topic. You mean that a Westerner, reared with scientific and logical thinking, cannot communicate with someone, say a Chinese, brought up in a different thinking pattern ?'

Mr Zeeland : 'Inasmuch as the "Western" thinking is based on communicable thoughts, you may succeed in re-educating the Chinese and making him learn "scientific" thinking by communicating with him. But even after he has learned "scientific" thinking the incommunicable part in him, if he has any, remains incommunicable to you. He may learn all your thinking. But you never learn all his thinking. That's the trouble. This applies not only to the case of the Chinese, but also to the case of those Westerners, say some Danes, or even some neurotics in your country, who have much of "incommunicable reality".'

Mr Able : 'But human beings are born with the same human nature. It is only through cultural experiences that they become different. Then, it should be possible, either by communicating one's experiences to others or by making them go through the same experiences as one's own, that one can make oneself understood.'

Mr Zeeland : 'There are difficulties both in the direct participation and in participation substitution by communication. As for direct participation, you cannot re-stage the Hungarian revolution in Sweden, so that the Swedes may participate in it and understand it. And as for participation substitution by communication, you have to keep two things in mind : (1) that " scientific thinking " is the product of adult thinking in the western cultures ; and (2) that the personality is formed mainly in childhood in the pre-logical and pre-scientific stage. Therefore in order to think like a Siamese, you have to first get out of your scientific thinking, go back to your pre-scientific and pre-logical infancy, and start all over again along the Siamese line of personality development. Now, the process of personality development is irreversible, just as much as some thermodynamic processes are irreversible. Though born with the same human nature, when brought up differently, people become different and the difference is never completely overcome by " communication ". You can analyse the Siamese character with your logic and scientific thinking. But that is different from becoming a Siamese. When an adult learns a foreign language or foreign customs, he does not start from " tabula rasa ", but he substitutes a foreign word or a foreign custom for a concept or a behaviour in his own language and culture, and gradually modifies the discrepancies he happens to discover. He is never free from bias, distortion, reduction, psychological projection, vicious circle of reasoning, and all his " scientific " and unscientific thinkings. An electronic brain can attain " tabula rasa " by erasing all the stored information. But it is built with a specific " logic ", and it can never go into the pre-logic stage. Therefore the electronic brain won't help you either .'

Mr Able : ' But as long as two human beings have some experiences in common they can start from there and build up complete mutual understanding, the limitations being due to " practical " factors such as channel capacity, inadequacy of coding systems, etc. rather than being of a " theoretical " nature .'

Mr Zeeland : ' There are theoretical limitations, too. From the point of view of the logic and mathematics, the intersection D of two closed systems A and B is closed. That is, the common domain of experiences between two persons is again closed. By starting with common experiences and common logical operations, you never get out of the common domain of experiences. The unknown parts of each other remain unknown to each other. However, that is when the procedure is strictly logical. With the help of more flexible procedures, such as intuition, you might perform something like " analytic continuation " of the theory of functions in complex variables, in which the domain of convergence of a power series, which represents a given function in that domain, may be continued outside the boundary of convergence by choosing another power series with another centre of expansion, which coincides with the first series in the common domain of convergence, but does not converge in all of the domain of convergence of the first series. In this way you may attain the understanding of the unknown. This last possibility is worth investigating '.

MAGOROH MARUYAMA

REVIEWS

THE IMPACT OF *LOGIK DER FORSCHUNG*

It is a great pleasure to be able to read this splendid book[1] in English with many additions and extra comments that yet further enhance its value.

A book that sets out the methods used in a particular subject must appeal to a wide variety of readers. In the present case it should appeal both to working scientists and to those who are especially interested in the logic of science and in the general workings of the human mind. A reviewer finds himself necessarily in one of the categories and few are able to claim membership of more than one of these groups, or even familiarity with the literatures of these fields. As working scientists the present reviewers fall into a very definite class. However, as scientists whose work has sometimes been criticised on philosophical grounds we may perhaps claim a slightly greater acquaintance with the philosophical problems involved than many others. With these preliminary remarks it is possible to say straightaway where the extraordinary appeal of this book to the reviewer lies: it rings true. It is rare for any human activity when analysed and dissected to bear a noticeable resemblance to that activity itself, but in this book Popper speaks as a working scientist to the working scientist in a language that time and again comes straight out of one's heart.

The characteristic feature of scientific theories which he emphasises and which has been much neglected by other philosophers of science is their continual development—the fact that they can be criticised and superseded by better ones. The empirical basis of science has nothing 'absolute' about it. He compares the structure to a building on piles in a swamp. We cease our attempts to drive the piles deeper, not because we have reached firm ground but because they are firm enough to carry the structure for the time being. Thus, he has no sympathy with the constructers of model languages, 'they miss the most exciting problems of the theory of knowledge—those connected with its advancement'. In fact scientists can never use a language system, for the language changes with every new step they take. The meaning of a concept is a function of the constantly changing theory.

Popper's discussion of induction is particularly satisfactory because it is so closely in keeping with the views of the working scientist. There is no such thing as induction, and so there is also no 'problem of induction'. His main argument is that physical laws are not in fact derived by an inductive process. A statement like 'all material bodies are composed of corpuscles' is quite obviously derived in an entirely different way from those delightfully simple examples ' all swans are white ' which we never find in any scientific treatise but only in the works of philosophers of science. At the same time he considers with the utmost tolerance various theories which tackle the non-existent problem of induction. In particular, conventionalism is very gently handled by him. The conventionalist, he says, seems to wonder at the beautiful simplicity of the world as revealed in physical laws. He seeks to explain this simplicity by regarding the laws as our own free creations and not falsifiable by observation. There is then no problem of induction, but what has been lost is made clear in a quotation from J. Black: ' A nice adaptation of conditions will make almost any hypotheses agree with the phenomena. This will please the imagination but does not advance our knowledge.' Since there is no induction the problem remains of characterising a *scientific* statement, and Popper clarifies this by discussing it as the problem of demarcation between science and metaphysics. He makes it quite clear that the previous attempts to discuss this problem failed to draw such a line, and for the most part—in their anxiety to annihilate metaphysics—they annihilated natural science as well. His criterion is not such a device for eradicating an unwelcome subject, but a proposal for an agreement about the subjects to which we attribute various arguments. The crucial step in his setting up of this criterion is the demand that any scientific system may be refuted by experience.

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[1] K. R. Popper, *The Logic of Scientific Discovery*, Hutchinson, London, 1959, p. 480, 50s.

At many places in the book one is struck by the aptness of the phrase, and one remembers occasions when it would have been most advantageous to have had the book available. As an example, it might be mentioned that at a recent conference on cosmology the steady state theory was attacked on the grounds that since the days of Galileo and Newton it has been a cardinal principle of science that a theory once established is not to be abandoned unless experimental evidence against it is found. However good this may have sounded, it was an incorrect statement. While there are not many occasions in science when a theory has been abandoned for other reasons, this is not in the least unheard of. The best reply to this criticism would have been to quote from Popper's book (pp. 53 and 54): ' Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop out without "good reason". A "good reason" may be for instance: replacement of the hypothesis by another which is better testable; or the falsification of one of the consequences of the hypothesis'.

With uncanny insight Popper put his finger on the point which is so very relevant to the case under discussion, the introduction of the steady state theory. For here the correct argument has always been that the steady state model was the one that could be disproved most easily by observation. Therefore, it should take precedence over other less disprovable ones until it has been disproved. Twenty years before the introduction of this hypothesis, Popper formulated the conditions with the utmost accuracy.

Another point which struck the eye of one of us was on page 107 on the relation of theory to experiment. Not only does Popper point out very strongly the heavy theoretical content of any so-called observation (a view that is rather more common now, chiefly owing to Popper's advocacy), but he also defines the task of the theorist more clearly than has ever been done before: It is his duty to formulate his questions as sharply as possible. Throughout the discussion of experimental work the author shows a magnificent awareness of the inseparability of theory and experiment, of how much the one is entirely dependent on the other. To bring this fact out in clear language is one of the great merits of this volume.

The scientific method for Popper is then the method of critically testing theories and selecting them according to the results. This method is governed by certain rules, for instance that ' the game of science is in principle without end '. Again, amongst auxiliary hypotheses we only allow those whose introduction does not diminish the degree of the falsifiability of the system. Thus Popper avoids the error of the positivists ' of trying to kill metaphysics by calling it names '. Indeed, he is well aware of the enormous importance of metaphysical speculation for the advance of science. It is true that some metaphysical ideas have obstructed scientific advance, but in general scientific discovery is impossible without faith in ideas of a speculative kind which may be sometimes quite hazy. It is from this raw material that we can forge the scientific theories which may then be hammered on the anvil of falsifiability. Thus in his opposition to induction, in his notions of testability, in his view on the problem of demarcation, Popper faces on every turn the problems against which the scientist runs up, and solves them in a logically satisfactory way in full accordance with the actual procedure of science. The notion of falsifiability in particular is a concept of the most direct significance to science.

It is difficult to single out any particular part of the book for special praise. With the exception of Chapter 8 (Probability), which is beyond the reviewers' field of understanding, there is a universal level of excellence that makes the book almost desirable bed-time reading. At the same time, one is occasionally a little perturbed by the strongly polemical style. The quotations from other authors that are so often mercilessly attacked frequently look as though they might just as well have been ignored as they are so clearly wrong. However, not being fully acquainted with the philosophical literature, the need for this amount of refutation may have escaped us.

It is rare for a reviewer to be able to be full of enthusiasm for a book, but this is one of the cases.

H. Bondi
C. W. Kilmister

THE STATE OF ECONOMIC SCIENCE[1]

In these three essays[2] Professor Koopmans offers what he calls ' . . . one man's explanations of some recent developments in economic theory, his comments and perplexities about the character and basis of economic knowledge, and his intuitions about possible directions of future work in theory and in empirical investigations.' The first essay, on 'Allocation of Resources and the Price System', is the explanation; Professor Koopmans provides a short course in set theory in order to exhibit some new work. This is fun; one may doubt if the re-working of a very familiar field with the new tools has yielded much of importance, but the tools are most attractive and are clearly displayed. The second essay, on 'The Construction of Economic Knowledge', raises problems of methodology which will be the main subject of this review. In the third essay, on 'The Interaction of Tools and Problems in Economics', Professor Koopmans uses his tremendous knowledge of recent developments in mathematical economics and econometrics in the U.S.A. to write a fascinating account of the new problems and opportunities. We shall quote one of his examples later, and we may find in it a clue to an obscurity in the second essay.

Koopmans' essay on methodology is, if I understand it correctly, open to two main criticisms. The first is that, compared with the position reached by an earlier writer on the methodology of economics, Koopmans appears to takes a step backwards. The second is that he does little to clear up our outstanding difficulties. These criticisms depend upon a view of the methodological debate in economics: the discussion of Koopmans must therefore be prefaced by a brief history of this debate.

Lionel Robbins' classic *Nature and Significance*[3] may conveniently be taken as a starting point. His methodology may be exhibited by quotation:

> The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and *indisputable* facts of experience relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality. The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production

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[1] Many of the arguments of this review I owe to the current school of thought among some of my colleagues at the London School of Economics. I am particularly indebted to Dr J. Agassi, Mr K. Klappholz, and Dr R. G. Lipsey.

[2] Tjalling C. Koopmans, *Three Essays on the State of Economic Science*, McGraw-Hill, 1957, pp. xi + 231, $6.50

[3] Lionel Robbins, *An Essay on the Nature and Significance of Economic Science*, London, 1932, 2nd edn. 1935. All references here are to the 2nd edn.

> is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. *These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everday experience that they have only to be stated to be recognised as obvious.* Indeed, the danger is that they may be thought to be so obvious that nothing significant can be derived from their further examination. Yet in fact it is on postulates of this sort that the complicated theorems of advanced analysis ultimately depend. And it is from the existence of the conditions they assume that the general applicability of the broader propositions of economic science is derived.[1]

On the problem of explaining consumers' behaviour, and the circumstance that their valuations of goods are subjective, he writes:

> . . . we do in fact *understand* such terms as choice, indifference, preference, and the like in terms of inner experience.[2]

And, on ' Economic Generalisations and Reality ':

> In Economics, as we have seen, the ultimate constituents of our fundamental generalisations are known to us by immediate acquaintance. In the natural sciences they are known only inferentially. There is much less reason to doubt the counterpart in reality of individual preferences than that of the assumption of the electron. *It is true that we deduce much from definitions.* But it is not true that the definitions are arbitrary.[3]

It appears now that the propositions of economics are deducible from ' obvious and indisputable ' postulates (or definitions). Hence we are not concerned with their testability, much less with their testing. Experiment is not merely difficult: it is irrelevant, unnecessary. Consider this passage:

> According to pure monetary theory, if the quantity of money in circulation is increased and other things remain the same, the value of money must fall. This proposition is deducible from the most elementary facts of experience of the science, and its truth is independent of further inductive test.[4]

After the ' Nature and Significance ', the next major contribution was Hutchison's. He insisted that the criterion of testability be rigorously applied to existing economic thought, and that there must be more room in economics than Robbins and others had allowed for looking at the facts in relation to theories. This was an advance; but Hutchison was unfortunately vague on the question of what we tested. He appears to have thought that it was in testing the assumptions of economic theory that empirical work was required.[1] At about the same time, a group of Oxford economists, conscious of the need for more facts, were acting on it: they were endeavouring to test the realism of such assumptions as that businessmen endeavour to set price and output so as to maximise their immediate, short-run profits.[2] Their method, roughly speaking, was to ask businessmen what they did, and why.[3] The answers they got were, roughly speaking, that businessmen did not try to maximise profits: they sought a ' fair profit ' or a ' normal profit '; they determined price by adding to the cost of raw materials and labour a mark-up to ' cover overheads and a reasonable profit '—except when market conditions prevented them.

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[1] Lionel Robbins, *An Essay on the Nature and Significance of Economic Science*, London, 1932, 2nd edn. 1935, pp. 78–79. Italics mine. T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory*, London, 1938, p. 131, quotes other economists to much the same effect, and deals thoroughly with them. Koopmans quotes the same passage from Robbins, and offers, as we shall see below, an interpretation slightly different from that suggested here.

[2] p. 88. Italics in original.

[3] p. 105. Italics mine. The individual preferences in question, incidentally, mean not just that individuals prefer some things to others, but that they have a complete and consistent ordering of all conceivable bundles of (completely divisible) goods, which ordering is independent of prices and of the preferences of others.

[4] p. 117. Robbins continues that the correct application of the proposition depends on correctly perceiving what is to be regarded as money.

The results were widely interpreted as meaning that prices were formed by adding a pre-determined mark-up to the prime costs of manufacturing. All the loose ends and loop-holes were pointed out (the frequent alibis in the form of reservations about ' market conditions ', the indeterminacy of ' normal ' or ' fair ' profit, the indeterminacy of the mark-up to cover overheads unless expected sales at the given price had already been calculated), but still ' this is what businessmen say they do '. In the debate that followed some people argued that businessmen could not maximise profits because they had never heard of marginal cost and marginal revenue (terms used in the economist's verbal translation of the mathematical solution of a simple maximisation problem); others, that since profit maximisation was the only ' rational ' thing to do, businessmen ' must ' maximise profits. The attempt to check the realism of the postulates led only to a methodological schism in economics: on the one hand we had those who paraded their ' realism '—' this is how businesses actually work '—and were indifferent to the arguments that their theory was indeterminate and therefore irrefutable; on the other hand we had those who stuck to ' rational ' theory, and appeared more and more indifferent to reality (as understood by the first group). We still suffer from this sterile and muddle-headed dispute.

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[1] There is little discussion in Hutchison's book of what and how we test, but what he does say lends itself to this interpretation. He writes of the need for ' . . . assumptions more nearly descriptive of the economic life of a contemporary community . . . ', and of finding out, ' . . . by the most extensive statistical investigation, precisely what these assumptions are . . . ' (p. 74); of ' . . . empirical verification . . . ' of the ' fundamental assumption ' (of maximising behaviour) (p. 83); he argues that, because existing value theory ignores uncertainty, it is ' inapplicable ' when individuals are uncertain what behaviour would maximise their returns (p. 89); and he concludes that, for a more ' realistic ' analysis, we require more ' realistic assumptions ' (pp. 119-120). Hutchison's intention may have been to urge us to compare predictions with observation; but, if so, he did not make himself sufficiently clear.

[2] See particularly the paper by Hall and Hitch reprinted in *Oxford Studies in the Price Mechanism*, ed. Wilson and Andrews, Oxford, 1951. The Oxford work was, I think, undertaken before the publication of Hutchison's book; but it is convenient to take them together because they are alike in insisting, both that facts and theories be studied in close relation to one another, and, as I interpret them, that the rôle of the factual study is in checking the descriptive reality of the assumptions of the theory. Some of the Oxford studies, however, were concerned with testing or measuring functional relationships such as the interest-elasticity of investment. The criticisms made here of the ' Hutchison-Oxford ' position are not meant to apply to these studies.

[3] There have been many criticisms of their technique, e.g., of the sample chosen, with which we are not concerned here.

It was left to Professor Friedman [1] to tell economists that the proof of the pudding is in the eating, that a hypothesis is to be tested by comparison of its implications with observation, and that descriptive unreality in the assumptions is not a defect but a necessity. Friedman's revolutionary essay (1953) was an attempt to state, in the context of economics, some principles of scientific method which have been current in the natural sciences since, I understand, Galileo. But economics is difficult, and there are difficulties in Friedman's essay, so we still have plenty of problems. The most serious ones are the problems of how we test, and what criteria we have for a good test. It is here that Friedman is most disturbing in his apparent complacency over what has already been done. The example is again the theory of the firm, the subject of much of the Oxford enquiry discussed above. It is extremely doubtful, in my opinion, if the theory of the firm has *ever* been the subject of really serious testing.[2] There are two common problems Friedman ignores. First, how is any theory which is purely static to be tested at all? The hypothesis, strictly interpreted, says that adjustments take place with zero time-lag, which we know is false. How ' unstrictly ' can we interpret it, and still have a refutable hypothesis? Second, there is the problem of the ubiquitous *ceteris paribus* clause. If the ' other things ' are not specified in advance,[3] there is an alibi for any refutation. If they are, we at least have a chance of learning from a refutation that the hypothesis must be widened to include some ' other things ' as variables.[1]

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[1] Milton Friedman, ' The Methodology of Positive Economics ', in his *Essays in Positive Economics*, Chicago, 1953. Since I criticise Friedman below, I should like to acknowledge now the great debt I owe to his writings on methodology.

[2] I except the work of the late L. Rostas on the ' cost-plus ' hypothesis (*Productivity, Prices and Distribution in Selected British Industries*, National Institute of Economic and Social Research, Cambridge, 1948), at least one version of which he appears to have refuted decisively; but his work on the profit-maximising hypothesis is less satisfactory.

[3] The list of ' other things ' is obviously infinite. The point is to specify which of them in particular are constants in the hypothesis in question, *and to ensure that they are observables.* ' Other things ' which are to be held constant in economics commonly

On the testing of the theory of the firm, Friedman writes: [2]

> An even more important body of evidence for the maximization-of-returns hypothesis is experience from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted. This evidence is extremely hard to document; it is scattered in numerous memorandums, articles, and monographs concerned primarily with specific concrete problems *rather than with submitting the hypothesis to test. Yet the continued use and acceptance of the hypothesis over a long period, and the failure of any coherent, self-consistent alternative to be developed and be widely accepted, is strong indirect testimony to its worth.* The evidence *for* a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its very nature is difficult to document at all comprehensively. It tends to become part of the *tradition and folklore* of a science revealed in the tenacity with which hypotheses are held rather than in any textbook list of instances in which the hypothesis has failed to be contradicted.

The question is whether this is good enough. My own opinion is that it is not. Consider:

(i) The age of a hypothesis, and the absence of a ' widely accepted rival ', are arguments that have been advanced in defence of practically every serious error ever made.

(ii) Is it not precisely those hypotheses which are wrapped in the cotton wool and authority of ' tradition and folklore ' that most urgently require testing?

(iii) Whether or not an economist *intended* to test a hypothesis is certainly irrelevant if, when he ' used ' it, he *in fact* did so in such a way as to expose it to test. But ' use ' alone proves nothing, and we may suspect that an investigator who is trying to refute a hypothesis is more likely to expose it to a serious test than one who is not. And, after all, for every test that would refute a hypothesis, there may be several that would not. We might suspect that Friedman is here looking for ' confirmation ' rather than refutation. Some documentation of serious tests conducted by accident would be really useful.

This passage I take seriously because its purport is to encourage complacency and to discourage that sceptical re-examination of the allegedly obvious that is the prerequisite of progress. In the preceding passage on the theory

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include such non-observables as ' tastes ', ' expectations ', or ' the state of business confidence '. A hypothesis constructed like this can never be refuted. Cf. Hutchison's discussion of the *ceteris paribus* clause, op. cit. pp. 40-46.

[1] This point is made by Hutchison, p. 44.

[2] p. 22. Italics mine, except *for*.

of the firm, Friedman is also, I think, mistaken, but the mistake here is less dangerous. He says, in effect, that we have the following evidence for profit maximisation: let businesses behave in any way at all; those which do maximise profits will flourish and those which do not will not; hence we know from ' natural selection ' that profit maximisation is a good hypothesis. Consider:

(i) Profit maximisation is consistent with losses (i.e., loss minimisation). The profit maximising hypothesis says nothing about capital structure, technical innovation, or, in fact, about survival at all.

(ii) To survive it is only necessary to be as well adapted as your rivals; it is not necessary to be perfect.[1]

The main criticisms of Friedman are, then, (*a*) an omission—he says nothing about the criteria for a good test, and (*b*) complacency—in consequence, perhaps, of (*a*), he far too readily accepts hypotheses as tested. One would feel better had he, if only once, said something to indicate that he thought that testing in economics was very difficult, and that it was hard to say what hypotheses, if any, had really been exposed to a critical test. There are other difficulties, but we may leave these for the moment.

It is against this background that I propose to discuss Koopmans' contribution, which means taking his argument quite out of the order in which he develops it, and fitting it into my own scheme (to which I add the warning that I find parts of his essay obscure: my interpretation may well be mistaken). My two main criticisms, that he takes a step backwards, and that he does nothing to clear up our difficulties, can now be reformulated: where Friedman made an advance, Koopmans ignores him; where Friedman was wrong or inadequate, Koopmans gives little help.

Koopmans' complaint against Robbins is that Robbins believed the major propositions of economics to depend on well-tested premisses when, in fact, these premisses are inadequate; he criticises Robbins, not for failing to state that the conclusions required testing, but for wrongly believing that the premisses were inadequate. His complaint is, therefore, of ' casual empiricism ', which should now be replaced with ' . . . direct or indirect testing of postulates '. Thus he refers with approval to Hutchison ' . . . who sees in the recognition of uncertainty the turning point beyond which *empirical verification of postulates should become the main preoccupation of*

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[1] Darwin seems to have anticipated this sort of misuse of his argument. He wrote: ' Natural selection tends only to make each organic being as perfect as, or slightly more perfect than, the other inhabitants of the same country with which it comes into competition . . . *Natural selection will not produce absolute perfection*, nor do we always meet, so far as we can judge, with this high standard under nature.' (*The Origin of the Species*, Everyman edition, p. 187, italics mine.) I am indebted to Dr Lipsey for pointing this out to me.

economists.'[1] This appears to throw the whole business back where it was before Friedman, and to re-open the door to the debate which followed the Oxford enquiry. (There is, however, another interpretation of Koopmans which will be suggested below.) We must now consider two respects in which Friedman left things unclear. He was concerned to show that descriptive unreality in the assumptions is both necessary and a virtue: the object of science is to predict from incomplete knowledge of the state of the world, and we can only discover what we can leave out of the hypothesis by our experience with its implications. It might have helped had he added that, even if assumptions were testable in any sense, and had been tested, and our deductions were logically correct, this would be no guarantee of the usefulness of the conclusions. It is not even a guarantee that the conclusions have testable content, much less that they will survive testing, because, obviously, we may have omitted important variables; the only way to find out is to test the implications. Friedman might also have avoided the confusion caused by the word ' assumptions ' in economics. In a passage which Koopmans criticises, Friedman writes [2] that the difficulty of testing in economics leads people ' . . . to suppose that hypotheses have not only " implications " but also " assumptions " and that the conformity of these " assumptions " to " reality " is a test of the validity of the hypothesis *different from* or *additional to* the test by implications. This widely held view is fundamentally wrong . . . '. Friedman is right to put the word ' assumptions ' in quotation marks, but it is a pity that he did not explain why. Economists use the word without the slightest regard for the logical status of the ' assumption '. Consider some examples:

(1) ' Assumptions ' about motivation, of which we have two important cases:

(*a*) Profit Maximisation. The alternative to testing implications here is, presumably, the notoriously unreliable method of asking people about their motives. Businessmen commonly reply that profit maximisation is not their motive. Friedman tells us to test implications, and believes, on what I think is insufficient evidence, that the implications of profit maximisation have stood up well to test. Koopmans, apparently believes that we should enquire further into the ' descriptive accuracy ' of the assumption, but does not tell us how to improve on the Oxford method.

(*b*) Utility Maximisation. Perhaps introspection convinces some people that they endeavour to select those purchases, among all those their incomes make possible, which will yield them the greatest utility; but such introspection does not convince everyone, its results not being interpersonally testable. It is hard to see what those who want to 'test postulates' can do in this case except explain their intuitions and invite others to do likewise.

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[1] p. 150. Italics mine. The difference, if any, between Koopmans' interpretation of Robbins and mine is not important. The important difference is over what we think should be done about it. Koopmans' agreement with Hutchison is also expressed in a footnote, p. 132.

[2] Op. cit. p. 14. Italics in original. See Koopmans, pp. 137-142.

(2) Empirical 'assumptions' of the existence and stability of functional relations, which we do want to test and measure directly. This testing is no guarantee that later conclusions are correct; but it may serve to remove an avoidable error (compare the discussion of input-output models below, and Koopmans' suggestions for improving them).

(3) The 'assumption' that certain things are constant, or 'may be neglected' for the moment. This may be a case of (2) above; but it may also be a device for temporarily limiting the field of an analytical study, e.g. 'I shall discuss the effect of a customs union on the terms of trade, *assuming* that the union is accompanied by no changes in technique, monetary policy, fiscal policy, . . .'.

(4) 'Assumptions' may also be used to explain the problem to which the hypothesis is meant to refer. New 'testing assumptions' may mean asking if the case at hand is one the hypothesis was designed for.

The many uses to which the word 'assumption' is put in economics have obscured the debate over whether we can 'test' them, or, if we can, if it establishes anything. Koopmans does nothing to clear this up. In spite of the care and rigour of his earlier analysis he appears, in his discussion of Friedman, simply to substitute the term 'postulate' for 'assumption' without any clarification whatever.

This interpretation of Koopmans, then, is that he follows Hutchison in calling for 'empirical verification of postulates' (or assumptions), and that he has done nothing to clear up the ambiguity of the terms. This interpretation gains support from the passages in which he criticises Friedman.[1] He points out that every statement implies itself, and that, in any case, '. . . in many systems of related propositions, one has a certain freedom of choice as to which statements one wishes to regard as premisses, and which ones as derived implications.' Hence, he appears to argue, the 'assumptions' which, according to Friedman, cannot provide an alternative test, can be turned into implications which, according to Friedman, we must test. Thus it is suggested that we have 'direct' implications and 'direct' tests of the postulates—'their accuracy in describing directly observed individual behaviour'; and that there is no reason for exempting some implications from test. Later on,[2] when Koopmans speaks of '. . . the intentional ignoring of obviously important aspects of reality . . .', there is no direct reference to Friedman, but we may conclude that Friedman is his target. Later, too, he writes of '. . . exhibiting the postulational basis, *and thereby the ultimate observational evidence*, on which our statements rest.'[3]

[1] pp. 138-40

[2] p. 143

[3] p. 144. Italics mine.

There are some rather obvious comments.

(1) We always, necessarily, 'ignore aspects of reality'. Our object, I take it, is to discover what aspects we can afford to ignore, or, better, what aspects we cannot ignore for a given purpose.

(2) Both Friedman and Koopmans point out that we have some 'freedom of choice' as to which statements are to be regarded as premisses and which as conclusions. What neither of them points out is that the choice is dictated by the problem at hand. If we want a hypothesis to predict prices, we require an implication about prices, which we shall test against our observation of prices. Perhaps Koopmans is thinking only of the second type of 'assumption' discussed above, that of a stable functional relationship between variables, which we do require to test. Such a relationship may well be the 'conclusion' of one part of our theory, and the 'assumption' of another part.

(3) 'There are fairies' implies 'there are fairies'. What of it? The trivial circumstance that a statement implies itself does not of itself make anything testable. Some 'assumptions' are empirical, and may be 'wrong' (although probably the best way to find out how much and for what their 'wrongness' matters is to test predictions based on them); some assumptions are non-empirical, i.e. non-testable in any terminology one chooses to employ, and will not in this respect be altered by their being made implications of some other statement. What 'direct implication' of utility maximisation is testable? (Intuition provides no inter-personal test.)

(4) It is not clear what 'direct' and 'indirect' tests mean in Koopmans' usage. Does Koopmans mean that 'profit maximisation' is a 'direct' implication of profit maximisation, which is to be 'directly' tested by asking businessmen if this is their motive? When they deny that this is their motive, does Koopmans regard this as a refutation of the hypothesis? He gives us no help. (In this connection it is startling to find that Koopmans approves Friedman's 'natural selection' argument that we criticised above.[1])

There is one other puzzling aspect of Koopmans' writing which we may notice here. He appears to see a conflict between rigour and realism[2] which I find hard to understand. The apparent realism, achieved at the expense of rigour, by the 'inextricable intermingling of facts and reasoning', is a harmful illusion against which Koopmans has warned us.[3] It appears that he is again worrying about the descriptive unreality of postulates which are framed with sufficient precision to permit rigorous analysis. Thus in his discussion of the theory of choice in conditions of uncertainty of von Neumann and Morgenstern,[1] he writes:' . . . the situations described in the model are, at least in their most direct interpretation, still so far removed from those met with in reality that verification by observation of actual economic decisions appears difficult, whereas verification in experimentally created conditions may well be of limited relevance for explanatory theory.[2] He overlooks the fact that Friedman and Savage,[3] and Markowitz,[4] have treated the von Neumann-Morgenstern theory as a positive hypothesis about behaviour, and compared its implications with observations about the purchase of insurance and lottery tickets. That I think there are implications they overlooked, and that these implications will lead to a refutation, is beside the point: the point is that the implications of the theory can be tested whether its formulation is rigorous (and postulates ' unreal ') or non-rigorous (' realistic '?); if it is rigorously formulated it is, of course, likely to be easier to discover just what it does imply.

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[1] p. 140. It is only fair to point out, however, that he suggests other implications and other evidence, and concludes that the profit maximisation postulate should be replaced with an appropriate postulate about survival policies.

[2] See, e.g. pp. 142-143

[3] p. 144. Koopmans writes an excellent warning against false optimism based on the apparent realism of non-rigorous intuitive argument

It was suggested above that there was another possible interpretation of Koopmans' essay. To find this we must turn to the sections in the third essay on ' Computing and Predictions of Production Potential ' and on ' Statistical Inference and the Measurement of Economic Behaviour '. We shall find here, too, what Friedman omitted entirely, and what Koopmans omitted from his essay on methodology—some discussion of the criteria of a good or a critical test. Here, in fact, he shows us a fascinating example of some problems in testing. It will be most convenient to set out the problem, and Koopmans' discussion, first, and then to see if his discussion suggests a reinterpretation of his earlier remarks on methodology.

The problem is to find a decisive test for an input-output model. This model is constructed, very roughly, in the following way. The industries of the economy are grouped into a number of ' sectors ', the output of each of which is treated as homogeneous. Each sector purchases materials from some others, and the ratios between these inputs, and the output of that sector, in the base year, are then taken as fixed coefficients. If the desired composition of final output (i.e. investment, exports, personal consumption, defence, etc.) is specified, it is now possible to obtain from the model the output level required in each sector. The use of the model, then, is to answer such a question as ' is this rate of re-armament technically possible? ' But before it can be used, the model must be tested. The obvious test is to compare the predictions for a given year derived from the model with observations for that year. With no standards of comparison, however, it is impossible to assess the significance of a given error. Hence ' naive models ' are constructed, the predictions from which can be used for comparison. With a naive model, for example, we might simply extrapolate on the assumption that the ratio of each sector's output to total output is a constant. A ' semi-naive ' model has been constructed with which we predict each sector's output from multiple regression of that output on total output and on time. The results of comparison are, apparently, that the semi-naive model leads to the best predictions, and that the input-output model is no better than the naive model. This is disappointing; but Koopmans does not think that it is decisive. A decisive test, he suggests, would only be afforded by a major upheaval, such as war or serious depression, which drastically altered the composition of total output. So long as this changes slowly, he does not find it surprising that naive extrapolation yields quite good predictions. The situation seems to be, however, that both the input-output model and the ' naive ' model constructed for comparison depend on naive extrapolation, the former of constancy in the coefficients, the latter of constancy in the composition of final output. Which yields the worse predictions depends on which extrapolation is worse. The problem, which Koopmans does not discuss, is therefore to *predict which* extrapolation will be worse. This is not a problem that can be solved within the framework of either model. Koopmans goes on to suggest a way in which the input-output model might be improved. Its use involves treating as homogeneous commodities that are not, and all the different production processes that go on within each sector as though they were identical, which they are not. These are ' unrealistic assumptions '. Hence he suggests that the sectors be ' taken to bits ' in order that information available at the engineering level can be used to check the coefficients that are ' assumed '. Work on these lines is apparently going on.

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[1] J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behaviour*, Princeton, 1947

[2] p. 159. I think we might substitute ' testing ' for ' verification ' in this passage without making too much fuss.

[3] Milton Friedman and L. J. Savage, ' The Utility Analysis of Choices Involving Risk ', *Journal of Political Economy*, 1948, reprinted in *Readings in Price Theory*, American Economic Association, 1953

[4] Harry Markowitz, ' The Utility of Wealth ', *Journal of Political Economy*, 1952

We now come to the second possible interpretation of Koopmans. If, by testing assumptions, and not ' ignoring obviously important aspects of reality ', he meant such activities as improving estimates of coefficients, we might reinterpret much of what he says on methodology that was criticised above. A difficulty remains, however: there are several different uses of the word ' assumptions ', and this interpretation will not fit all of them. We should therefore suggest that Koopmans, thinking of a particular, and useful, case of ' testing assumptions ', had been led to a generalisation which does not fit the other cases, and is therefore misleading. If this is the case, then the difficulty lies in his having confused the point, overlooked by

Friedman, that *some* assumptions need testing, with the general argument of 'descriptive accuracy' versus 'testing implications'. Had Koopmans attempted some taxonomy of assumptions, we might have been saved both this confusion and our difficulty in interpreting his intention.

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Concepts of Force: A Study in the Foundations of Dynamics. By Max Jammer. Harvard University Press. London: Oxford University Press. 1957. Pp. x + 269. 45s.

A notable feature of modern history and philosophy of science is the abandonment of that kind of inductivism which undervalues the influence of conceptual ideas in the formation of theories and assumes that the truth is read off observations by the most empty minds and the most accurate instruments. There are still however too few studies of the development of the key concepts of science, and it is pleasant to be able to welcome this new work from Professor Jammer, who has already put us in his debt with his *Concepts of Space.*

With a wealth of historical detail and citation of source material, much of it not easily available, Jammer traces the vicissitudes of the concept of force from its mythological origins to the present day. A chapter on 'Ancient Thought' shows force as a very general notion moulded by analogy with ordinary experience and having animistic, mythological, and theological connotations. A chapter on 'Greek Science' shows the gradual disentanglement of 'force' from 'body' and its subsequent identification (by Plato) with soul, and (by Aristotle) with the power of self-movement residing in some bodies. The Stoic conception of forces as propagated universally by *pneumata* is then described and contrasted with the mechanical contact-action of Aristotle, and this is followed by a review of Aristotelian and Stoic influences in medieval and Renaissance science. In the work of Kepler we find the first quantitative discussion, and an explicit rejection of an animistic in favour of a corporeal notion of force as a result of his discovery that the velocity of a planet, supposed caused by the sun, diminishes with its distance from the sun. Next we see the classical mechanical ideas coming to birth in Galileo, Descartes, and Huygens, culminating in a detailed discussion of Newton's concept of force in the *Principia* and his speculations about an aetherial medium for gravitation. Regarding Newton's work as the watershed in the history of the concept of force, Jammer then leaves the discussion of technical developments in mechanics itself and devotes the next four chapters to various interpretations of the notion of force—the theological interpretations of More and of Newton's immediate disciples; the dynamism, that is the metaphysical account of force as a primary property of substance, of Leibniz, Boscovich, and Kant; the mechanistic view which led to many attempts to develop a corpuscular theory of gravitation; and the 'critical' analysis of force as a relational construct, from Berkeley to Mach. Finally there is a chapter on contemporary science in which brief sketches of the place of force in quantum, nuclear, and relativity theory are given, concluding with the possibility of its ultimate elimination in the projected unified field theories.

Jammer has sought to be comprehensive rather than to argue any specific theses, nevertheless form his assembled sources a general pattern emerges which might be called the 'life-cycle' of a concept. We see the concept of force growing up amid a welter of animism and metaphysics, its coming to maturity in Newton, its struggles even then to cast off the clinging hands of false metaphysical interpretations, its subsequent emancipation as a pure construct justifying itself solely in terms of the work it does in the correlation of experience, and its final supercession by younger concepts (energy or space-curvature) and death, its work being finished. That the book falls into this pattern is due to the fact that Jammer has chosen to take as definitive of 'force' what appears under that name at a particular stage in the history of physics, namely in the *Principia Mathematica*. Now obviously some method of selection has to be adopted in writing history, and this is a legitimate one, but it does suffer from certain disadvantages. In the first place there is the temptation to read the 'pre-history' of the concept wholly in terms of a fumbling after things half-realised, and to look for the main significance of early science in its supposed anticipations of later, successful, theories. On the whole Jammer's detailed and sympathetic accounts of Greek and medieval thought avoid this temptation and are given with a real feeling for historical context, but there are some jarring notes. I am not convinced for example that the true flavour of the Stoic theories of *pneumata* is conveyed by comparison with the action at a distance of classical physics, nor that justice is done to the real originality of Aristotelian mechanism, in comparison with which Stoic theories of force were a reversion to the primitive. Again, Jammer's interpretation of Roger Bacon's 'multiplication of species' as analogous to the propagation of a wave, and as containing some inkling of conservation of energy, obscures the wholly non-modern character of this theory, in which transmission of the image of an object to the eye, and of magnetic force, is comparable with the reproductive propagation of an animal species. Jammer is careful to explain the linguistic variations and ambiguities in the word 'force' from the Greek *dynamis* to the nineteenth-century German *Kraft*, but he surely has the cart before the horse when he suggests

(p. 13 and elsewhere) that ambiguity of terms *led* to confusion of thought. It is not that precise senses distinct in themselves had to be disentangled from vague and ambiguous words, but that the precise senses had to be *created* by discovering the theories in which they are used. The history of the distinctions made between 'force', 'energy', 'momentum', 'work', *is* the history of mechanics.

Another danger inherent in tracing the history of a particular concept rather than a perennial problem (for example, 'how do bodies act upon one another ?'), is that its metaphysical accompaniments must seem in the end to be accidental and dispensable. The impression is also given that the progress of science in general (not merely the concept of force) is (in Comtean fashion) from an animistic, through a metaphysical, to a positivistic stage, and this does indeed seem to be Jammer's philosophy of science, as outlined in his first chapter. Here he remarks that today science does not offer 'a photographic picture and true image of reality' (p. 2), but only a description of experience and general principles under which experience can be subsumed and by means of which predictions can be made. The resulting theories are described in terms of a conceptual deductive system and its rules of interpretation into the data of experience, and Jammer remarks that the choice of concepts is *arbitrary*, since various systems could be made to work by changing the rules of interpretation. Thus the history of a concept in its formative period is the study of the *extra-scientific* influences which led to one conceptual scheme rather than another. Now if this were true, it would surely be unnecessary to enter into these accidental formative influences in order to understand a concept in modern physics—only an understanding of the formal relations within the deductive system and its interpretations would be necessary, and it is indeed in this belief that most scientists consider the history of their subject to be fundamentally irrelevant. Jammer clearly does not believe this, for one of his declared aims is to address himself to 'the frightening discrepancy between our technical "know-how" and our philosophical incomprehension . . . [which] seriously endangers the integrity of our intellectual outlook' (p.v.), and he believes that his historical-critical analysis will meet this need. His insight into the importance of historical background is not consistent with his explicit philosophy of science.

Jammer has, I think, been misled by a false analogy between the development of a single concept and the conditions of progress of science as a whole. Of course if attention is concentrated on a concept whose sense and use is fully determined by its place in a finished theory, such as the concept of force in classical mechanics, then its metaphysical ancestry and contemporary interpretations will be irrelevant, but, as clearly emerges from Jammer's account, the concept is then as good as dead and ripe for elimination. What does not clearly emerge from this way of treating the matter is that the various metaphysical interpretations of Newtonian force were productive of *new* concepts, for example fields of force and energy. How can Jammer assert (p. 187) that 'the conception of force as the primordial element of physical reality . . . was not very fruitful and productive for the advancement of theoretical physics', when only three pages earlier he has described its influence on Faraday, and has previously discussed the Leibnizian concept of energy? A similar confusion arises in connection with the mechanistic theories of gravitation. Because Jammer regards the 'critical' view of force as a relational construct as not merely reflecting its use at a particular period of its history, but as the only ultimately correct view for this and any other concept, he considers that 'any attempt of a mechanistic theory to replace . . . this functional concept of force by the assumption of a chain of causal contiguous actions, was doomed to failure' (p. 249). This is both to underestimate the influence of a metaphysical demand for contact-action (shown for example in modern field theory) and to mistake the positivist *interpretation* of theories for a *prescription* as to what kind of theories they shall be. Even if the positivist interpretation is accepted, there is no reason why theories involving spatio-temporal contiguity should not be more satisfactory than action-at-a-distance theories. It should also be remarked that the concept of cause in general is not to be identified with the concept of force, even though this identification may have been reasonable in the time of Kant, and the elimination of force from physics does not imply the elimination of cause.

Finally, is it true that the concept of force in modern physics merely 'plays the role of a methodological intermediate comparable to the so-called middle term in the traditional syllogism' (p. 244)? Jammer thinks that a reconstruction of force in this sense will eliminate the metaphysical, *a priori*, intuitive and anthropomorphic elements which he claims (pp. 121, 124, 162, 209) to find in Newton. But Newton here shows more insight than most of his commentators. In the *Principia* (end of Scholium to Definition VIII) Newton's argument is this: if two bodies are accelerated relative to each other there will be observable effects of a definite acceleration in one or both of them (tension in a rope or concave surface of water in a rotating bucket). Hence acceleration is an absolute property of a moving body. Hence we can identify a system of inertial frames (Newton thought one of these was primary, but this is here irrelevant). Newton now *defines* force as that which produces acceleration, in his Definition IV (since he explicitly makes this a definition it is somewhat gratuitous to assume as Jammer does on page 124 that after all he meant to do something else). Hence force can be said to be an absolute cause of the acceleration. Nothing in the theory of relativity has altered this situation, for we do in fact distinguish inertial from accelerated frames, however the difference between them may be explained. That this is so is shown by the following example: suppose we observe two mutually accelerated bodies A and B of which only A shows internal effects of acceleration (e.g. two trains in one of which the passengers feel their backs pressing against the seats). Then we can interpret the situation in two ways: either as the acceleration of A, or as the effect of a force acting on both bodies creating an acceleration effect in A (which is actually stationary) and exactly cancelling out the effect in B (which is actually accelerating). Now if there are no other *bodies* in the vicinity which could explain the force in the second interpretation *we always prefer* the first interpretation. This shows that we regard force (the ability to produce acceleration) as a property only of bodies. Hence, although the mathematical sign for force may be a mere intermediate and eliminable term, the interpretation of this term is not. We must know what kinds of entities exert what kinds of forces even in order to calculate accelerations, and Jammer tacitly admits this (p. 244) when he defines force in terms of configurations of masses, charges, magnets, etc. In this sense, and in this sense only, our (and Newton's) concept of force remains absolute.

If I have found matters of philosophical interpretation to criticise in this book, that should not detract from our gratitude to Professor Jammer for his highly stimulating and informative study.

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Metaphysical Beliefs : Three Essays.
By Stephen Toulmin, Ronald W. Hepburn, and Alasdair MacIntyre.
S.C.M. Press, London, 1957. Pp. 216. 25s.

Mystery and Philosophy. By Michael B. Foster.
S.C.M. Press, London, 1957. Pp. 96. 12s. 6d.

ONE of the major sources of modern linguistic philosophy has been the philosophical analysis of science. From Mach through the early Russell and Wittgenstein to the Vienna Circle and many of its successors, science has appeared to be at once the most important domain of use of human language, and also the least complicated in its logic, and a primary concern of philosophers has been to characterise factual statements as those which are potentially scientific, and to disentangle these from other forms of discourse : metaphysical, emotive, imperative, recommendatory, and the rest. This programme has borne most hardly upon metaphysics and theology, since these, alone of the non-scientific forms of speech, appear at least *prima facie* to be fact-stating. Professor Popper has recently pointed out that, when he suggested that the criterion for scientific statements is falsifiability, it was not with the intention of dismissing metaphysics as meaningless, but of distinguishing it from science. But the question remains of how and whether metaphysics and theology can be said to have meaning and what if any are the criteria of their validity.

It is remarkable that it is theology rather than metaphysics that has recently staged a comeback into the arena of linguistic philosophy with the appearance of several books of which two are here under review. It is refreshing to find in them a serious concern to grapple with the philosophical questions raised by the existence of religious, and particularly Christian, beliefs, by writers who are also at home in the worlds of science, literature, and philosophy. *Metaphysical Beliefs* contains three essays: in *Contemporary Scientific Mythology* Professor Toulmin discusses the quasi-religious myths such as the Running-Down Universe and Evolution (with capital letters) which, he argues, are illicitly derived from genuine scientific hypotheses; in *Poetry and Religious Belief* Mr Hepburn describes the uses and abuses of an analogy between poetic and religious language; and in *The Logical Status of Religious Belief* Mr MacIntyre argues that religious language must be taken as it is used and on its own terms and neither distorted in the interests of empiricism nor judged in terms of logical categories foreign to it. In *Mystery and Philosophy* Mr Foster makes a radical distinction between the sphere of science, which is under man's control, and the sphere of 'mystery', by which he means that which can be known or obtained only as a revelation or gift of God.

The relation between these essays and the philosophy of science is, I think, twofold. In so far as scientific knowledge is in some way taken as standard, it is interesting to see how the authors assess the claims of other kinds of knowledge or bodies of doctrine which do not seem to deserve the label 'scientific'. Underlying this assessment is the account of the scientific 'standard' which is presupposed, and though there is little direct discussion of this, it is clearly important to see whether the science with which theology, poetry, and metaphysics are implicitly compared is adequately characterised. I shall therefore start with some comments about this.

From Professor Toulmin and Mr Foster we get a fairly detailed account of what they conceive science to be, the concerns of Mr Hepburn and Mr MacIntyre are different, nevertheless there is the explicit assumption in all four writers that scientific knowledge is empirically testable and that this provides it with well-understood truth-criteria. So far there will be general agreement. Toulmin and Foster further provide us with a picture of scientific theories doing a modest and workmanlike job, enabling us to exercise our proper function to 'replenish the earth and subdue it', but offering no profundities of either hope or despair: agnostic and Christian here unite in playing down the role of science in the formation of beliefs. Let us look more closely at Toulmin's account.

He uses three criteria for distinguishing what he calls 'scientific myths' from theories proper:

> 1. In theories technical terms (' evolution ', ' entropy ', ' gravity ') have a clearly defined place and a limited application. In myths such terms are used with vaguely extended meanings which have no warrant in the theories from which they arise.
>
> 2. Hence what is being said in the myths has no definite testable content, and there are no scientific means of deciding between contradictory myths. If T. H. Huxley sees human evolution as a struggle against the predominant features of natural evolution, whereas Julian Huxley finds in natural evolution the justification for the best of human values, the question as to which is right is not a factual question. Neither view has any basis in science itself.
>
> 3. This means that scientific myths are put forward for reasons which are not scientific but temperamental, social, or political, and from these generic causes they draw their power to convince.

This is an admirable analysis, and the only quarrel I have with it lies in the assumption that it is always clear how scientific terms and theories should be used and what are their limits of application. This can never be the case in a developing science, as I think Toulmin would readily agree, but I suspect that his agreement would be to put up with a bad job, rather than to accept as an essential characteristic of science that it is not a work of logic but of the imagination, having fuzzy edges where we are not clear what we are saying or how far it is testable, and indeed are too much interested in searching for imaginative schemes of explanation to care much for definition and absolute clarity. Who would claim that the notion of fundamental particles is ' defined in a way which is closely tied down to the phenomena ', or that its task ' is to explain some limited range of phenomena ' (pp. 20, 21) ? On the contrary, *we do not know* what a fundamental particle is, and yet we wish to claim much for it and to say that such theories as we have about it are of *universal* application. At this point Toulmin would no doubt reply ' Yes, but in the long run we are able to decide which of these imaginative speculations to retain and which to reject, because if they are to be scientific we must discipline them by introducing definition and testability '. Here I would agree. Nevertheless I still suggest that testability, specially in the most fundamental and most interesting theories, may be very indirect in the long run, and the fact that it is necessary for the progress of science that speculative schemes should sometimes be admitted into it means that no sharp line can be drawn between what can and cannot be in principle scientific. Toulmin does not sufficiently consider the flexibility given to scientific theories by the use of ' models '. For example, when the thermodynamicist is bent, in spite of Toulmin's cautions (p. 34), on applying the Second Law to the universe, he does not say ' The universe is an isolated system ', for this by itself, as Toulmin rightly argues, is not a statement to which any testable sense can be given. What he says rather is ' Let us consider a model universe in which we assume that no heat energy is introduced except what is derived from other forms of energy within the system '. This model can then be elaborated, and testable consequences derived from it, so that it is in this sense scientific, although as with most cosmological models it is unlikely that it will be possible to eliminate all its rivals, and therefore it will remain tentative as a description of the facts. That it may be *wrong*, however, does not imply that it is not an attempt at factual description, and whether it is scientifically interesting will depend on all kinds of considerations related to the state of physics which are more complicated than the simple logical criterion of testability would suggest.

Before leaving this point it may be remarked that a more liberal interpretation of the relation between factual content and testability, at which I am hinting, may do something to bridge the gulf which Mr Hepburn finds between image and existence in his discussion of Jungian archetypes. If the theory of archetypes is a psychological model of dubious testability, it is in similar case with cosmological and some other physical models, and this in itself need not prevent us according them existential status. Many other questions arise here, and Mr Hepburn discusses them in detail, but at least there is no need to be inhibited by an over-strict view of what it is to be factual in physics.

Toulmin, however, has another argument which leaves me unclear whether he wishes to give any realist interpretation to scientific theories at all, even when they are testable. He rightly complains in regard to cosmological theories about the Beginning and the End that such extreme extrapolations are often talked about with undue confidence, just when they ought to be handled with the utmost caution, and this seems to imply that he admits the factual (' historical ') status of such extrapolations. But later (p. 41) he suggests that we can compare theories to map projections which are inter-transformable, and which in themselves say nothing, but only enable us to order our experiences in different ways. Now this instrumental view of theories is a widespread one and cogent arguments can be advanced on its behalf, but it is also beset by well-known difficulties. If Toulmin wishes to hold that in spite of the difficulties theories are like this, then none of them can be said to state any facts over and above the phenomena, and it is hard to see why he distinguishes between theories and myths on grounds of meaning and possible truth-content. On such a view all non-formal theories would be myths, but some myths would be more useful predictive devices than others.

Let us turn to Mr Foster's view of science. For him it represents all that is merely human and under human control, all that is communicable to and repeatable by man as such without aid of divine grace: ' Science will be applicable to all things which are subject to man. This means, it seems, to *all* things. . . . What does this leave outside the limits of science? God ' (p. 61). But for Foster this implies that all that is mysterious and unspecifiable, all that is not univocal, and all that offers ' depth after depth to the understanding without being exhausted' (p. 54) is left outside science. But I think what I have said above about the role of the imagination in science indicates that this is not necessarily so. Even I would suggest that (as Professor Polanyi has often pointed out) the practice of science does require a kind of 'repentance of the intellect', in the sense that certain decisions about values have to be made in it. Foster's characterisation of science does not however lead him to undervalue it, and he has an interesting comparison of the Greek and Hebrew thought-models in which the spirit of modern science is seen to be more congenial to the latter than to the former. (He has developed this elsewhere, but I cannot find that he has referred to it in his book, so I will do so: *Mind*, 1934, p. 446; 1935, p. 439; 1936, p. 1.)

To say that science has after all some features in common with myth and metaphysics does not, however, appear to give the religious believer nearly as much as he wants, for whatever science may be potentially, it is certainly not now talking in the categories of religion. What then are we to make of religious language? Here I can only briefly indicate how our four authors proceed. Toulmin gives a generic account of scientific myths which is convincing enough, but he does not appear to think such myths can be more than wish-fulfilments. Hepburn finds illuminating analogies between religious and poetic language which help to show how religious assertions can be meaningful and possible, but he is dogged throughout by the insistent question: Very nice, but is it *true*? MacIntyre and Foster are in broad agreement in their method of dealing with this question, namely, that there is no *logical* transition from non-religious to religious language, but only a revelation whose reception involves repentance and for which no scientific evidence nor technique is possible (Foster), or a conversion, engrossment by a passion, which convinces the believer of the content of a faith, and after which criteria of the truth of any particular doctrine will be looked for *within* that faith and in terms of its recognised authorities (MacIntyre).

> 'Of science and morals it can also be said that one can justify particular theories or prescriptions, but that one cannot justify science as a whole in non-scientific, or morals as a whole in non-moral, terms. Every field is defined by reference to certain ultimate criteria. That they are ultimate precludes going beyond them.' (MacIntyre, p. 202.)

In the last analysis perhaps it is true that our contemporary puzzlement is due to the fact that in the twentieth century we do not in general find ourselves accepting unselfconsciously the ultimate of religion (i.e. belief in God), whereas we do accept in our bones the empiricism of science. If that is indeed the situation then logic is powerless. But it is also powerless, though no doubt in a different way, to lead us from one fundamental scientific theory to another. We have to wait on inspiration. But even if belief in God is the ultimate stumbling-block in this matter, the suspicion remains that since some kinds of scientific language are similar to some kinds of religious language, and since the forms of expression of religious ' revelation ' or ' authority ' are in any case not static, logic and empiricism may be more relevant to the passage from science to religion than is allowed by the ' wholly-other ' view of religion presented here.

The debate is an old one, and will no doubt go on a lot longer. These books are contemporary contributions to it of a very high standard.

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Sigmund Freud, Life and Work. Vol. 3: The Last Phase. By Ernest Jones. The Hogarth Press, London, 1957. Pp. 536. 35s.

Most people today would readily admit that physics is not necessarily the only theory according to which we must fashion all the other sciences. But until recently there were no other theories which differed significantly from physics, so that they could provide us with another model. The so-called ' living ' sciences are not sufficiently developed; and, in fact, most of them—like biology and physiology—are mixed sciences, in the sense that they deal also with ' inanimate ' facts and not only with living processes. Biology, for example, must include chemistry and so, from the theoretical point of view, it cannot furnish us with as sharp a distinction between the physical and the non-physical sciences as we might wish. This does not hold true of psychology. If one were to range the sciences according to a scale indicating how much of the ' human element ' they contain, then one would place physics at one end of the scale and psychology at the other. Psychology is the most human of all the sciences—as Freud himself once remarked—and this is possibly one reason why its scientific status is so often doubted and certainly the main reason why it is the youngest of them. It appears to be an impossible task to contain the vagaries of emotional and mental life of a human being within the rigid framework of a theory. No generalisation and no universal law are possible if we all differ from one another as much as we think we do.

As long as psychologists investigated the superficial behaviour of a person, there was little hope for making progress towards a scientific psychology. Indeed, paradoxical as it sounds, psychology could not become a truly scientific discipline while it remained closely bound to physics in its methods. Almost the only law that was discovered during this stage of psychology was that of Fechner; it was rightly called by its discoverer a law of psycho-physics. Most academic psychology, including behaviourism, has remained within this narrow ambit since only phenomena connected with conscious perception and intellect are found worthy of investigation—phenomena, that is, that are nearly independent of human emotion. ' Public ' facts that can be described in terms of the space and time of external, physical reality are considered to be the only raw material for a science. The motion of a permanent thing in space-time is alone accepted as observable, while the emotion belonging to inner, psychical reality is rejected as a merely introspected, ' private ' datum. Thus it was exactly the assimilation of psychology to physics that prevented its progress, since only the superficially visible, conscious phenomena were studied.

With the advent of psycho-analysis progress was made, namely, when Freud described the mechanisms and processes within us of which we are unconscious but which, all the same, determine our conscious feelings and overt behaviour. This ' microscopic ' description is needed for constructing a scientific theory of psychology. In physics the macroscopic description leads to a good, though, it is true, limited theory which is then succeeded by various microscopic theories. The macroscopic view is almost totally useless in psychology, since what human beings do must be explained in terms of what they feel and not vice versa. *Si duo faciunt idem, non est idem.*

In this final, third volume of the biography, Dr Jones gives an historical account of the main contributions Freud has made to the various sciences, including psychology (see the review of the first two volumes in this *Journal*, 1956, **7** p. 25). What is most interesting to the philosopher of science is to find out how far, if at all, the psycho-analytic explanations differ from those in other fields. After all, scientific method is not thought out *a priori* but is given by the rational reconstruction *ex post facto* of actual theories. There is no reason to believe that the kind of explanation we accept must be the same in every science, apart from common logical characteristics that arise from the use of language. Any explanation must obviously depend on the kind of phenomena it treats. Freud himself wanted, for a time, to model psychology on the physical sciences of physiology and neurology, until he was forced by clinical observation to introduce different concepts and methods which are better fitted to explain the emotional life of human beings.

We are determined, simultaneously, both by what we were as children and by what we are experiencing now. The genetic method of explanation was introduced into science already by the theory of evolution and it is needed for describing the historical, or developmental, aspect. The dynamical type of explanation is needed for describing the continuously changing processes which go on in ourselves though we are unconscious of them. The basic ambivalence of emotional life, with its shifting equilibrium, must be pictured as a back-and-forth movement: it is an oscillation between opposite poles or an interaction between opposing forces. This interaction as well as the forces themselves change all the time. We have here a truly dynamical process in which nothing remains constant for any length of time, in contrast to physics where we have constant forces and even permanent things. A living system, moreover, is open, unlike the closed system of physics. This ' loosening up ' in the actual phenomena and their increased complexity resulting from it must be reflected in the theories explaining them.

This is shown up in Freud's metapsychology—the ' account of mental processes that comprised a description from a dynamic, topographical, and economic point of view ' as Dr Jones characterises it. A mental event must be described on various levels according to the superego-ego-id differentiation; a quantitatively different composition of the various instincts produces a qualitatively different human character; and the event must be explained both in terms of the past and of the here-and-now. The ' vertical ' explanation of genetics is often suspiciously like that of *post hoc, propter hoc*—which is contrary to everything we ever accepted as causality. Yet the repetition compulsion to which we are all subject and which arises from the periodic characteristics of life seems to be of this type. The ' horizontal ' explanation of dynamics is even more difficult, since the process cannot be cut up (as in physics) into two forces or things acting against one another. But we have no proper ' process-language ' and so we must do with the thing-language of physics and every-day life. This limitation, I think, is mainly to blame for the lengthy and futile discussions about Freud's assumption of a death-instinct. Apart from the personal objections we all feel, it seems to be too facile an assumption, too simple a dualism. Certainly, we must distinguish the death-instinct's working from the destructiveness arising from frustration. But if we want to describe a process, we cannot do without a dualism, in the simplest instance; there must be at least two agents, factors, poles or whatever you want to call them, between which the process occurs. Once a life-instinct is assumed—and no-one seems to object to this—the death-instinct enters automatically, as it were, for logical reasons. Obviously, the best thing is not to describe human existence in such general terms: the dualism can be expressed more specifically, for example, as the opposition of male and female, or of love and hate, etc. Still, a question remains: since the cells of our body and we ourselves die and there is nothing we can do about it, why should there not be an instinct in our emotional life that reflects this biological drive ?

There are, then, a great many differences between an explanation in psycho-analysis and, say, in physics. Whatever one may say against specific formulations, psycho-analytic theory as a whole has been undoubtedly successful in many fields, and it has put psychology on a scientific basis. The revolution that Freud has brought about has been generally recognised and even welcomed, for instance, in the arts. In science, that is, in what we take as the scientific attitude and the method that represents it, this revolution has not yet been sufficiently appreciated. What we accept as truth and reality does depend, however, on what we know about ourselves and not only on what we know about the external world. Indeed, the two kinds of knowledge are inextricably connected. The biography which Dr Jones has given us will prove to be invaluable in teaching us this new understanding of the microcosm and macrocosm that we owe to Freud.

Ernest H. Hutten

Physical Science and Physical Reality. By L. O. Kattsoff.
Martinus Nijhoff, The Hague, 1957. Pp. 311. 35s. 6d.

This book approximates to the state of the primal universe which, we are told, was 'without form and void', which fact makes the reviewer's task rather difficult. It is perhaps best to adopt a teleological approach. What is an author's purpose in writing a book on, say, the philosophy of science?

One can think of three possible aims. First, he may write his book as some artists paint their pictures and some musicians compose: merely to express his personality. Though the work may be of value to others, its real merit lies in its psychological effect on the author. To a reviewer who has never met the author, such merit is impossible to assess.

The second purpose is to present new facts or theories, or at least new interpretations, to the reader. This would include manifesting material not easily accessible, by reason, say, of language difficulties. There is little or nothing of this kind in the present book.

Thirdly, the book may be intended for students, i.e. the uninitiated, young or old, who require a digestible summary of the subject. It is in this category alone that the book can be considered.

Let us try to view the book through the eyes of a student. First, he will find an admirable bibliography for his further reading, British authors being particularly well represented. (In some cases brief summaries of the contents are included.) But one is led to suppose that the author has read all these books himself and then, during an attack of acute mental indigestion, has attempted to reproduce their substance. For example, the student will find pages, almost intact, from text books on physics, with the equation '$s = \frac{1}{2} gt^2$' repeated as a kind of refrain, until he cries out 'enough: no more'.

Most of the rest consists of harmless enough discussions on the classical problems of the philosophy of science. From time to time the author appears to get completely 'bogged down' and the student, if he is alert, will say to himself 'Yes, but surely . . .' and discover a simple way out of the morass, only to be disappointed that the author has found the same path in the next chapter but one, having wandered into several other bogs in the interval.

One example will suffice. How confusing is the status of imaginary experiments requiring, for example, ' weightless bars and massless strings '! We give up and decide that such things must be quite different from real laboratory experiments until twenty pages later, a slightly inaccurate account of the effects of lubricants in reducing friction between solids brings illumination: in the limit, we can reduce dynamic friction to zero even though the perfect experiment is impossible.

One case only will be quoted where a happy solution never seems to emerge. If, from long experience, we have concluded that all swans are white, what are we to do when someone finds a bird in all ways similar to a swan, but black? I will not solve this one, in case some student wants to work it out for himself; but here is a hint: what about a white blackbird?

There is, I think, only one major issue on which the student is likely to be seriously misled: but this is important enough to warrant brief discussion. I refer to the author's treatment of Operationalism, which, I feel, shows serious misunderstanding.

The operational definition of length as measured with a ruler is duly described and the contradistinction between this and the older concept is stated: ' Length is usually taken to mean a measure of separation and not a set of rules for obtaining this separation '. But, in many cases, the measure of separation cannot be made by means of a ruler and triangulation is used. Length measured by triangulation may or may not be the same as ruler-length, depending on our geometry. The point at issue with the operationalists is whether, in certain cases, we should adjust the geometry, or whether we prefer to define two different sorts of length which, we are pleased to find, very often agree with one another.

The author's treatment of definition of temperature is even more confused. ' If a term is synonymous with a set of quantities, then it is reasonable to expect that one be able to replace the term by the set of quantities. . . . Suppose we measure the temperature of the water in a glass and our thermometer reads 48°, then if we say the temperature of the water is 48°, we are saying ' the 48° of the water is 48° '!

In fact, the history of temperature measurement provides rather a strong case for operationalism. As an illustration, in 1946 it was reported in the Proceedings of the Royal Society that from $-183°$ to $+660°$ C, temperature was to be defined in terms of the resistance of a platinum wire; and from 660° to 1063°, by the e.m.f. of a standard thermocouple. Now it so happens that pure aluminium freezes at just over 660°, using the resistance thermometer, thus falling within the range of the thermocouple; but, when the thermocouple is used, the freezing temperature is just below 660°, so that the resistance thermometer is indicated. Until a suitable adjustment in these conventions was made, it was impossible to quote an accurate freezing point for aluminium. We may *define* temperature in terms of kinetic energy of molecules (though it is hard to agree with Kattsoff that it is *caused* by their kinetic energy). But, unless we agree on a method of measurement, we cannot quote a figure for a freezing-point, which at least leads us to think about the advantages of an operational approach.

I have reviewed this book at some length mainly because it seems likely, from its title, that it will come to the notice of many readers of this Journal. The title is, indeed, somewhat misleading, since only a few of the chapters deal with the significance of 'physical reality'.

As a further minor criticism: the proofs have been carelessly read, and there are many small misprints.

G. W. Scott Blair

Space, Time and Creation. By Milton K. Munitz.
The Free Press and the Falcon's Wing Press, Glencoe, Illinois, 1957.
Pp. xi + 182. $3.75.

In 1954-55 the author of this book, who for some years had been giving a course in cosmology ('Theories of the Universe') at New York University where he is an Associate Professor of Philosophy, was awarded a Ford Faculty Fellowship to write an up-to-date philosophical critique of the methodology of scientific cosmology. The result is a very sane and useful book which should take its place alongside the late Susan Stebbing's *Philosophy and the Physicists* and Dingle's Eddington Memorial Lecture as a permanent contribution to the small number of illuminating books in this field. Although it lacks the brilliance and wit of the older works, it is equally readable, and as its scope is wider it ought to be read by all who are interested in cosmology.

The author begins with a brief survey of the historical development of cosmology from ancient myth to modern science. (It may be mentioned here that Dr Munitz has also recently edited a useful historical anthology *Theories of the Universe*.) This survey is followed by a chapter on the nature of scientific theory in which he outlines his philosophy of science. To use Professor Popper's terminology, he seems to incline towards 'instrumentalism' in so far as he regards a scientific theory as essentially a pattern of thought which may be used in interpreting facts. But, like Professor Popper, Dr Munitz does not believe that a theory can be arrived at by pure induction. He defines a 'model' as that form of specification of a theory which is so constructed that it will be used in the account of a specific range of subject matter. In particular, he applies this definition to theoretical world-models. Consequently, when in chapter V he comes to consider the idea of the ' universe ', he argues that, although in a general sense it can be said to be the concern of the cosmologist in the same way as ' life ' is the concern of the biologist, ' mind ' of the psychologist and ' matter ' of the physicist and chemist, in a more specific sense the theoretical cosmologist means by this term ' a theory of the universe '. Therefore, contrary to what we have been told so often by cosmologists, the universe is *not* unique. This seems to be a fundamentally sound point and one which the reviewer himself suggested during a Discussion on Some Philosophical Problems of Cosmology at a Group Meeting on 12th June, 1950. (*Bull. Brit. Soc. Hist. Sci.*, **1**, No. 5, Phil. Sci. Group Supplement No. 4.) Munitz makes the further point that it is not axiomatic that we must restrict the concept ' universe ' only to what is observable *in principle*, e.g. ' inside ' the observer's light-cone. ' Whether or not, therefore, the universe is unique, from the point of view of the entire field of theory construction, is better formulated as : whether or not a particular cosmologic *theory* is acceptable. And to this, of course, one can never give any answer which will uphold the claim that one theory is *absolutely* superior to all others actual or possible ' (p. 93). Incidentally, when the author comes to discuss the ' orthodox ' relativistic world-models, he argues that the ' openness of possibilities ' is a virtue rather than a fault.

The later chapters are devoted to pertinent and acute critiques of the methodological standpoints of Milne, Einstein, and the Steady-State Cosmologists. (Curiously enough, Eddington is hardly mentioned by name and his cosmology is nowhere discussed. Lemaître fares little better.) The criticisms of Milne and of Hoyle and Bondi seem to me to be both just and to the point. Of particular interest, however, is the author's criticism of Einstein whom he likens to another great Jewish thinker, Spinoza. I found this parallel, which I admit had never occurred to me before, most illuminating, particularly in relation to Einstein's concepts of ' space ' and ' field '.

Dr Munitz argues that, instead of Einstein's view that nature is like a puzzle, it should be regarded rather as a series of problems.

> Puzzles, after all, are constructed *by* men *for* men, and we know in advance, by virtue of the fact that they are so constructed, that there is a solution. We legitimately look for it. But we have no reason to speak of Nature as a puzzle. We experience *problems* when we seek to order our experiences and we solve our problems when we get satisfactory ways of interpreting the data that poses [*sic*] them. But this is a relative matter. For comparative successes need not be graded on a single scale on the assumption that there is a supremely satisfactory solution (p. 117).

The final short chapter, entitled ' Scepticism ', contains apt rejoinders to Bridgman's operationalist criticism of the *raison d'être* of cosmology and also of Kant's unhistorical attitude (' such arguments reflect the use of evidence accepted at a given time ').

To sum up: the essence of the author's philosophy of cosmology is that the universe studied by cosmologists should be regarded *historically*. On the one hand, there is a growing mass of observational data; on the other, a number of theories for its interpretation. The very meaning of the term 'universe' is dependent on this many-sided activity and its results at different historical epochs.

G. J. Whitrow

ANNOUNCEMENTS

The British Society for the Philosophy of Science

The Philosophy of Science Group has now been reconstituted as the British Society for the Philosophy of Science.

A resolution of 12 January constituting the new Society was as follows:

> 'That a new Society be formed which shall have similar aims and constitution to the Philosophy of Science Group of the British Society for the History of Science, and that the name of the new Society be the British Society for the Philosophy of Science, and the new Society shall come into existence at once on 12 January.'

A resolution dissolving the Philosophy of Science Group was passed on 2 March:

> 'That the assets of the Philosophy of Science Group be taken over by the British Society for the Philosophy of Science and that the Group be dissolved forthwith.'

The activities of the new Society will in general be the same as those of the former Group.

The Annual Conference of The British Society for the Philosophy of Science will be held at Newnham College, Cambridge, from 25th-28th September. Details may be obtained from Miss M. B. Hesse, University College, London W.C.1.

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

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Bright, L., *Whitehead's Philosophy of Physics*, Sheed & Ward, 1958, pp. 48, 2s. 6d.

Brown, H. (Ed.), Deutsch, K. W. and others, *Essays on Science and the Creative Spirit*, University of Toronto Press, 1958, pp. xxvii + 165, 36s.

Coulson, C. A., *Science and the Idea of God*, Cambridge University Press, pp. 51, 4s. 6d.

Dixon, W. M., *The Human Situation*, Pelican Books, 1958, pp. 443, 5s.

Estes, W. K. & Suppes, P., *Foundations of Statistical Learning Theory*, 1. *The Linear Model for Simple Learning*, Stanford University, pp. vii + 99

Feibleman, J. K., *The Pious Scientist*, Book, am Associates, 1958, pp. 111, $3.00

Fothergill, P. G., *Life and Its Origin*, Sheed & Ward, 1958, pp. 77, 3s. 6d.

Gilbert, W., *On the Magnet*, Basic Books Inc., New York, 1958, pp. viii + 246 + 67, $8.50

Gotlind, E., *Three Theories of Emotion*, Ejnar Munksgaard, Copenhagen, 1958, pp. 151

Gruber (Ed.), Zirkle C., and others, *The Emergence of the Modern Mind*, Pennsylvania University Press, 1958, pp. 93, 20s.

Hansen, N. R., *Patterns of Discovery*, Cambridge University Press, 1958, pp. ix + 241, 30s.

Harris, Dale B., (Ed.) *The Concept of Development*, Oxford University Press, 1957, pp. x + 287, 38s.

Heisenberg, W., *The Physicist's Conception of Nature*, Hutchinson, 1958, pp. 192, 16s.

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Passmore, J., *A Hundred Years of Philosophy*, Gerald Duckworth, 1957, pp. 523, 35s.

Polanyi, M., *Personal Knowledge*, Routledge & Kegan Paul, 1958, pp. xiv + 428, 42s.

Pole, D., *The Later Philosophy of Wittgenstein*, Athlone Press, 1958, pp. 132, 15s.

Russell, J., *Science and Metaphysics*, Sheed & Ward, 1958, pp. 40, 2s. 6d.

Schrödinger, E., *Mind and Matter*, Cambridge University Press, 1958, pp. vii + 104, 13s. 6d.

Schrödinger, E., *Science, Theory and Man*, Allen & Unwin, 1958, pp. xxiv + 223, 18s.

Slichter, C., *Science in a Tavern : Diversions on Science in the Making*, University of Wisconsin Press, 1958, pp. ix + 206, $1.00

Speirs, H. L., *The Fluid Ether*, Centaur Press (distributors), Winterson, 1958, pp. 47, 7s.

Thorndike, L., *A History of Magic and Experimental Science*, Vol. VII, Columbia University Press, 1958, pp. x + 695

Thorndike, L., *A History of Magic and Experimental Science*, Vol. VIII, Columbia University Press, 1958, pp. viii + 806, 80s.

Warnock, G. J., *English Philosophy Since 1900*, Oxford University Press, 1958, pp. x + 180, 7s. 6d.

Weizacker, C. F. von, *Ethical and Political Problems of the Atomic Age*, SCM Press, 1958, pp. 22, 2s. 6d.

Winch, P., *The Idea of a Social Science*, Routledge & Kegan Paul, 1958, pp. 143, 12s. 6d.

Wittgenstein, L., *The Blue and Brown Books*, Basil Blackwell, 1958, pp. xiv + 185, 25s.

Wright, C., *The Philosophical Writings*, The Liberal Arts Press, 1958, pp. xxii + 145, 80 cents

(*b*) ARTICLES

Bergmann, G., ' Frege's Hidden Nominalism ', *Philosophical Review*, 1958, **67**, 437-459

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Builder, G., ' Ether and Relativity ', *Australian Journal of Physics*, 1958, **11**, No. 3

Gerard, R. W. (Ed.), ' Concepts of Biology ', *Behavioural Science*, 1958, **3**, No. 2, 92-215

Grünbaum, A., ' Fundamental Philosophical Issues in the Special Theory of Relativity ', *Kritik und Fortbildung der Relativitatstheorie*, 1958

Hartshorne, C., ' Freedom Requires Indeterminism and Universal Causality ', *Journal of Philosophy*, 1958, **55**, No. 19

Joja, Ath, ' Essai sur la nature de la deduction et de l'induction ', *Revue des Sciences Sociales*, 1956, **1**, No. 1, 3-63

Joja, Ath, ' Sur quelques aspects de la logique dialectique ', *Philosophes Roumains Contemporains*, 1958, 59-114

Kaila, E., ' Uber Das System Der Wirklichkeitsbegriffe Ein Beitrag Zum Logischen Empirismus ', *Acta Philosophica Fennica*, 1936, **2**, 101

Kaila, E., ' Uber Den Physikalischen Realitatsbegriff Zweiter Beitrag Zum Logischen Empirismus ', *Acta Philosophica Fennica*, 1941, **4**, 191

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Kaila, E., ' Terminalkausalitat Als Die Grundlage Eines Unitarischen Naturbegriffs Eine Naturphilosophische Untersuchung ', *Acta Philosophica Fennica*, 1956, **10**, 122

Kaila, E., ' Einstein-Minkowskin Invarianssiteoria ', *Ajatus Filosofisne Yhdistyksen*, 1958, **21**, 1-121

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Machacek, J., ' Soud a jeho druhy ', *Sbornik Praci Filosoficke Fakulty Brnenske University*, 1957, **6**, B4, 97-120

Madden, E. H., ' The Riddle of Induction ', *Journal of Philosophy*, 1958, **55**, No. 17, 705-718

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Passmore, J. A., ' William Harvey and the Philosophy of Science ', *Australian Journal of Philosophy*, 1958, **36**, No. 2

Riepe, D., ' What is a Scientific Naturalist at Mid-Century ? ', *Journal of Philosophy*, 1958, **55**, No. 17, 726-734

Scheffler, I. & Chomsky, N., ' What is said to be ', *Proceedings of Aristotelian Soc.*, 1958, **58**, 71-82

Watkins J. W. N., ' The Alleged Inadequacy of Methodological Individualism ', *Journal of Philosophy*, 1958, **55**, 390-395

Whitrow, G. J., ' El estudio de la filosofia de la ciencia ', *Suplementos del Seminario de Problemas Cientificos y Filosoficos*, 1958, S_4-67-68

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ERRATA

In the previous number in ' A Theory of Causality ' by I. J. Good, p. 310, the words ' note added in proof ' have been omitted immediately following the first three paragraphs, and should be inserted. Also, the two Is in line 19 should be vertical strokes.